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THE PSYCHOLOGICAL REVIEW

DESIRE, CHOICE, AND PURPOSE FROM A NATURAL-EVOLUTIONARY STANDPOINT

BY MORRIS A. COPELAND

Cornell University

The attempt at a logical application of the doctrine of scientific or natural evolution to man and to life gives rise to two of the major issues before psychology and the social sciences at the present time. These two issues are raised by the assumption—necessary to such an application of evolution theory—that there are certain general characteristics common to the observable physical structure and functioning of inorganic bodies, of the so-called 'lower' forms of life, and of man; and that these several types of structure and functioning shade off into one another by comparatively small differences[1] leaving no sharp lines or 'missing links' historically.

1. The first of these two issues is that of Behaviorism vs. Introspectionism. The peculiar characteristics of the human species that give rise to special psychological problems can, according to the simplest hypothesis consistent with a natural-evolutionary view, be understood as characteristic modes of bodily functioning. The mind-body relationship is conceived as a relationship between body functioning and body structure. Mind is a functional characteristic of the human body. Knowing is a form of physiological response to stimulus. The possibility of one man's observing another's mental processes, like the possibility of observing another's digestion becomes a question of developing laboratory technique. The

[1] In specifying 'small differences' it is not intended, of course, to make the differences so small as to conflict with a mutation theory.

alternative to this Behavioristic view must assume either (*a*) a sharp break in the evolutionary series when mentality first appears, or (*b*) a vast realm of 'phenomena' which, by the nature of the hypothesis, can never be directly observed. The former assumption is clearly in conflict with a natural-evolutionary view. According to the latter assumption the observational data necessary to a phylo- or ontogenetic account of mental states and processes are lacking, since 'introspection' by animals, infants, and some types of abnormals is impossible to obtain. If this latter psycho-physical dualist hypothesis is not to conflict with a natural-evolutionary view, it must not deny in the case of the human body the uniformity or regularity of physical processes and conditions upon which the possibility of science rests. In other words, the sequence of physiological and other physical processes and states must be assumed to be 'complete' for purposes of scientific investigation. This means assuming that it is possible to discover relationships between an organism's behavior and antecedent circumstances in the organism or its environment, which will enable one to predict and control the behavior through these antecedents, or specify the antecedents given the behavior, without resort to non-physical data. Mental states and processes, if non-physical, must be mere parallels or duplicates of physical conditions and events.

For the sake of simplicity the second issue, which is our present concern, will be discussed in terms of the Behavioristic hypothesis. In the nature of the case this treatment of desire, purpose, and choice should be equally capable of statement in terms of a psycho-physical parallelistic position, if that is consistent with a natural-evolutionary view. To translate the present discussion into parallelistic terms, it would be necessary to add the 'parallel' or duplicate mental states and processes.

2. The second issue has sometimes been called Teleology vs. Mechanism.[2] A natural-evolutionary view requires that

[2] 'Mechanism' here means assuming as a working hypothesis that the behavior of all living organisms is like the behavior of inorganic bodies in one respect, viz., that control is always from antecedent to consequent, and never conversely. To take such

human behavior should be accounted for scientifically by analyzing it into parts and formulating descriptive generalizations about these parts, that can be used for predicting and controlling behavior or specifying its antecedents. Teleological interpretations of behavior according to this view should be used not as 'explanations' which are substitutes for a scientific account where that has not yet been worked out satisfactorily. Rather, as will be argued below, they should be regarded as complements of such a scientific account.

The advocates of a teleological 'explanation' of behavior as distinguished from an objective description in the sense just suggested, have too seldom defined the concept precisely. Indeed, precise definition makes such a position difficult to hold. Apparently a teleological explanation is one in which a consequent is held to determine its antecedents. Thus behavior would be *explained* teleologically (where the behavior consisted of a number of responses, each more or less appropriate to a discoverable end, the responses being repeated or new appropriate responses following old ones until the end is achieved) by discovering the end which the antecedent responses subserve, the implication being that the end somehow selects or determines the responses appropriate to bringing it about.[3] An objective scientific description on the

a position as this, it is not necessary to suppose that we can now furnish a satisfactory analysis of apparent cases of the converse, control of antecedent by consequent, showing them to be instances of control from antecedent to consequent. It only requires us to continue to search for such an analysis. And the resemblance between organic and inorganic behavior in this one respect says nothing as to the resemblance between men and machines in any other respect. Other resemblances and differences are left open to empirical determination.

[3] The teleologist's statement of his position frequently confuses the issue of teleology vs. mechanism with that of introspectionism vs. behaviorism. Thus a teleologist may admit that the 'idea of the end' or result may precede the 'means' which brings the result about, so that the *antecedent idea* controls the subsequent behavior which in turn leads to the result to which the idea 'had reference.' On a parallelist hypothesis, the antecedent idea would 'parallel' a physiological condition or process which may also be said to control the behavior leading to the 'imagined' result, and the 'reference' of the 'idea' would have some physical counterpart, *e.g.*, resemblance between (*a*) the antecedent physiological parallel of the idea, and (*b*) some part of the end-result. But this is mere 'mechanism.' On the other hand, if we

other hand would always treat the antecedents as controlling or determining the consequents. Control is from stimulus to response, from antecedent to consequent, and not conversely. It is difficult to see how the advocates of a teleological *explanation* as a satisfactory substitute for scientific description in which control is held to be from antecedent to consequent can reconcile their view with a natural-evolutionary position.

If certain characteristic results or 'ends' of the behavior of primates enable us to specify antecedent responses which are 'means' more or less appropriate to bringing about these results—and probably both sides could agree so far—either (1) these generalizations about primate behavior useful for specifying and controlling these antecedents must be eventually capable of reduction to a form in which antecedent determines consequent, or (2) this organic behavior must be categorically different from inorganic behavior as it is known to physical science. In the first case the use of a teleological account of human behavior may be a useful temporary expedient, but the scientist will be continually looking for an account in terms of control from antecedent to consequent to fill in this logical gap in his system of thought. If one accepts the second alternative, one must [4] apparently suppose that organic behavior cannot have evolved as a special case of physical bodily behavior,—the teleologist is essentially a vitalist. Incidentally, to take this position is tantamount to jumping from the proposition, "Science has not yet filled in the logical gap so as to enable us to account scientifically for human behavior in which 'ends' appear to determine antecedent 'means,' by putting the analysis in terms of control

abandon parallelism, to the extent of leaving either the idea of the end, or its 'reference' or 'meaning' without a physical counterpart, the introspectionist definition of teleology (*i.e.*, one employing an antecedent 'idea' of the end, with a future 'reference') becomes identical with the one here advanced (consequent controlling antecedent) *so far as physical events and conditions go;* and in hypothecating non-uniformity of physical human nature as it necessarily does, it conflicts with a natural-evolutionary view.

[4] It is, of course, possible to take a supernatural evolutionary view, such as Hegel's or Bergson's, by holding that physical science deals with phenomena or appearances rather than reality, or by holding that scientific method can never grasp living reality.

from antecedent to consequent," to the proposition, "Science can never fill this gap." To accept the second alternative, then, is to come into direct conflict with the hypothesis of natural evolution. The issue between these two views is: Shall we proceed on the hypothesis that organic events in which consequent appears to determine antecedent can be accounted for in terms of antecedents determining consequents? The theory of natural selection is an attempt to render an account of phylogenetic 'adaptation' to the needs imposed by environment (selection of the means or adapted structure or functioning determined by the ends or environmentally imposed needs) in terms of control from antecedent to consequent. To account for human purposive behavior in such terms is as much [5] a necessity for the natural-evolutionist as to account for phylogenetic adaptation. It is the aim of the present paper to offer a tentative constructive formulation of a natural-evolutionary view of desire, choice, and purpose.

I

While McDougall is undoubtedly right in insisting on the importance of some teleological *interpretation* of human behavior,[6] that does not justify treating the teleological interpretation as a substitute for a scientific account. It may be well to consider first of all what part a teleological interpretation should play in a scientific treatment of human behavior.

The biologist is not satisfied with a mere description which can be used for prediction, specification and control. (And psychology and the social sciences are, according to a natural-evolutionary view, biological sciences.) The physiologist, for example, specifies the ends or functions which the various organs of the body serve; and this is part of his job as a scientist. Function in this sense is to be distinguished from purpose. The function of an organ is a result which the

[5] Embryology offers another difficult field for the natural-evolutionist.

[6] McDougall, W., 'Purposive or Mechanical Psychology?' PSYCHOL. REV., 1923, 30, pp. 273–288; also 'Purposive Striving as a Fundamental Category of Psychology,' *Scient. Mo.*, Sept., 1924.

observing scientist expects the organ 'normally'[7] to bring about and in terms of which he appraises its behavior. It is not an end which guides or influences that behavior. Thus the result expected of an organ is an interpretation persisted in whether or not the result actually occurs. As a teleological interpretation it does not purport to 'explain' or even describe the behavior of the organ, but rather it tells what the result ought to be. It still remains necessary to make a scientific description of the behavior of the organ, a description both of the normal functioning and of the functioning of the organ which fails to realize the result expected. Such a *description* is wrong if it leads to wrong predictions or specifications; but the correctness of a teleological interpretation in terms of function is not directly dependent on what actually does happen. It states what the interpreter thinks ought to happen even when it does not happen. It is no less truly the function of the stomach to digest food, when the stomach is 'out of order.' Hence such a teleological interpretation supplements but can never be a substitute for an objective description. *Appraisal of the behavior of an organ as appropriate to the performance of the organic function is not part of the description that makes possible prediction, specification, or control of behavior.*

But psychology's attention to the 'ends' of behavior cannot be confined to specifying the functions of reflexes and other reaction-patterns. The natural-evolutionist as well as the teleologist must recognize that men behave purposively or (to employ a term which may smack less of 'conscious' intent) telically: Ends are involved in their desires, choices, tastes, valuations, interests, volitions, etc. These 'ends' enable us to specify antecedent responses. Those antecedent responses appear to be selected which are appropriate 'means' to bringing about the 'ends.' The result which is the function of a response continues to be the function of the

[7] It might be urged that 'normal' means 'typical,' and is therefore useful for prediction, subject to a 'probable error.' But that 'normal' does not mean 'typical' is shown by cases where an abnormal or pathological condition is typical, *e.g.*, most adults have decayed teeth.

response whether it follows the response or not. But if the result which we suppose to be the purpose or desired objective of a response fails to follow, we conclude either that the response was a 'trial-and-error' or that we have misjudged the purpose or desired objective.

From a scientific or natural-evolutionary point of view human behavior which is popularly said to be dominated by a desire, or interest—telic behavior—cannot satisfactorily be understood simply by finding and specifying the ends (results) which such behavior serves to bring about. Purposive behavior by man is presumably a special instance of animal behavior, to be understood not in terms which are peculiarly appropriate to man, except as a temporary expedient. Rather it is presumably to be understood by somehow reducing these terms, peculiarly appropriate to man, to a form in which they appear as special instances of animal and if possible of bodily behavior in general. Thus one task which the natural-evolutionist is required to face is how to account for telic behavior or behavior in which a consequent appears to determine its antecedents in terms of control of consequents by antecedents, *i.e.*, in stimulus-response terms.

II

One who contemplates tackling this task must face the fact that the teleological view is fortified by long-established habits of thought that permeate the vocabulary of the anthropological sciences as well as that of common sense. Among the psychological terms which lend themselves to a teleological explanation may be listed: trial-and-error (a trial aims at an *end*, and an error fails to achieve it), reward and punishment (these consequents appear to affect antecedent associations in learning experiments), adjustment, adaptation, preparatory and consummatory responses, random movements, intelligence. In addition to these which can only be listed here there are two sets of terms which call for more extended comment.

1. Such terms as instinct, disposition, tendency, etc. illustrate the difficulty that many terms may in one context

involve teleological explanations, but not in another. Thus 'instinct' may be employed in an explanation of behavior in terms of an innate endowment of ends. But this teleological meaning for the term 'instinct' needs to be sharply distinguished from the meaning of 'instinct' which makes it a specific innate reaction pattern which is called out originally by a specific stimulus. In this stimulus-response sense an instinct, if it differs from a reflex, differs only in being more modifiable. Instinct in the teleological sense is likely to be defined in terms of the end toward which a variable set of responses proceeds,[8] or in terms of a type of processes subserving some single end. When 'instinct' is used in a teleological sense, moreover, it is common not to name the specific response pattern involved—there are usually a large number belonging not peculiarly to any one instinct, only a few of which actually function in any single 'expression' of the instinct.[9] It is also common not to name, or to pass lightly over the original stimulus which calls it out,[10] or else to define the stimulus in terms other than a specific quality of an object which arouses a specific group of receptors, *e.g.*, in terms of its meaning or significance for ends attributed to the organism by the interpreter.[11] Instinct in this sense is capable of being the basis for an 'explanation' of absolute values such as truth, welfare of the organism as distinguished from the

[8] Thus McDougall conceives an instinct as 'an innate disposition' which can be 'satisfied by the attainment of its natural goal,' *J. Abn. Psychol. & Soc. Psychol.*, 1921–22, 16, p. 300.

Note also the frequent use of the purposive infinitive in connection with particular instincts, *e.g.* 'the instinct to flee from danger.' 'Introduction to Social Psychology,' 1923, p. 51.

[9] McDougall believes we must accept the view that "instinctive action is distinct from any mechanical reflex action, and an instinct is not, and does not essentially comprise in its organization, any motor mechanism. . . . An instinct, or given instinctive impulses, may make use at need of a large array of motor mechanisms, different instincts expressing themselves on different occasions through the same motor mechanism; although it may be true that any one instinct most readily finds expression through some one such mechanism,' *J. Abn. Psychol. & Soc. Psychol.*, 1921–22, 16, p. 311.

[10] *Cf. idem.*, 'Introduction to Social Psychology,' pp. 60, 62.

[11] *E.g.*, "Among all animals that display pugnacity under any conditions, any *threat* to the young seems to be the surest excitant of such behavior." McDougall, W., 'Outline of Psychology,' 1923, p. 139. Italics by present writer.

person's opinions about what he needs, etc. Human nature takes the place of Nature with a capital 'N' as a basis for philosophizing. Moreover, instincts in this sense can be balked, expressed, perverted, or sublimated. But if they are to be sufficient in themselves for purposes of a teleological 'explanation' of behavior, they are presumably by and large unchangeable throughout the life of the individual; his ultimate ends are not modifiable by learning.[12]

2. Certain terms of the introspective or mental-states psychology may easily be used to lend support to a teleological explanation of conduct, particularly those designating the emotional and affective mental states. Thus pleasantness and unpleasantness may be defined, somewhat as they were by James Mill, as qualities of mental states which the organism tends to prolong or to end,[13] *i.e.*, of future mental states which the organism tends to seek or avoid. Thorndike's 'satisfiers' and 'annoyers' are practically translations of pleasantness and unpleasantness.[14] These states have been thought of as originally attaching to instinctive 'seeking and avoiding responses,' and transferred by education to non-hereditary 'responses' which in the nature of the case must be also 'seeking and avoiding responses.'[15] It is characteristic in connection with these mental states as well as with instincts to speak of expression and repression.

III

It is essential for a scientific or natural-evolutionary account of telic behavior (1) to distinguish carefully the

[12] Of course, a natural-evolutionary view requires them to be accounted for phylogenetically and ontogenetically in *non-teleological terms*. While McDougall does deal with the genesis of ends in a way, his attitude toward a thorough-going natural-evolutionary account is definitely hostile. He feels that psychology is not required to give such an account of the instinctive ends, for "to attempt to give any further account of the nature of these conative dispositions would be to enter upon a province of metaphysical speculation, and is not a task demanded of psychology. . . . At the standpoint of empirical science, we must accept these conative dispositions as ultimate facts, not capable of being analyzed or of being explained by being shown to be instances of any wider, more fundamental notion." 'Social Psychology,' p. 368.

[13] Mill, J., 'Analysis of Mind,' 2, chap. XVII, his terminology being pleasure and pain.

[14] Thorndike, E. L., 'Original Nature of Man,' 1913, pp. 123 *ff.*

[15] Dickinson, Z. C., 'Economic Motives,' 1924, pp. 140 *ff*, 158 *ff.*

job of making a generalized *description* of telic behavior such as to aid in prediction, specification, and control, from the *appraisal* of behavior in terms of ends or functions, so that the appropriateness of a response to an end, *qua* appropriateness, may not be taken as an 'explanation' of the occurrence of that response; (2) to avoid using teleological terms in scientific description as ultimate, unanalyzed concepts, since descriptive teleological concepts are peculiarly appropriate to the behavior of man and the more closely related animals, and distinctly inappropriate in the case of inorganic bodies; and (3) to put this generalized description of telic behavior into terms which will make the telic behavior of man a special instance of bodily behavior in general, in that for telic as well as for other behavior it will hold true that antecedents determine consequents rather than conversely. In other words the natural-evolutionist must seek to render a description of telic behavior which will make it possible to define terms like 'purpose' and 'desire' in terms like 'stimulus' and 'response,' must seek to understand behavior in which responses appear to be selected as appropriate to bringing about a certain result as a special instance of behavior made up of responses determined by their appropriate stimuli.

In this connection it is important to distinguish between 'telic' and 'teleological.' The word 'telic' applies to *behavior* in which antecedent responses appear to be determined by the consequent 'end.' The word 'teleological' applies to *terms* or *statements* which imply that consequent determines antecedent in telic behavior. It has seemed to some, perhaps because they have not clearly drawn the distinction, that to describe *telic behavior* in *non-teleological terms* somehow makes it no longer telic. This is precisely on a par with the view that to understand man and monkey as descendants of a common ancestor is somehow to dehumanize man. Man is no less man for being known as a primate, *nor is human purposive behavior less purposive for being understood as a special type of animal, and even of bodily, behavior in general.* Telic behavior itself is not altered by altering the description of it. The most that can be asserted is that a description in non-

teleological terms will always and inevitably be a wrong description—which is another way of asserting 'free will,' of assuming *human* behavior to lack the regularity or uniformity which makes a scientific description of it possible. Such a theory may be true—human nature may not be uniform enough to allow much prediction, specification, and control, but we are more likely to make progress in anthropological science if we assume as a working hypothesis more uniformity than has yet been discovered.

In beginning an attempt to understand telic behavior in non-teleological terms, it may be well to list the non-teleological terms which we have at our disposal: (1) reaction-systems or response-patterns; (2) association or conditioning —the alteration of a reaction-pattern so that new stimuli which have previously accompanied the original stimuli become appropriate to arouse the functioning of this reaction-pattern; and (3) the development of inhibition [16] or of the failure in the presence of an 'inhibitory' stimulus, of a reaction-pattern to function, although the appropriate stimulus is present.

The typical human being has a vastly greater number of response-patterns for which appropriate stimuli are present at any one time than could in the nature of the case function simultaneously, since many of the responses to these stimuli are mutually exclusive (*e.g.*, standing and sitting). We may speak of the organism as paying *attention* or being awake to some stimuli rather than others. At any one time he responds only to a relatively small number of the stimuli to which he has corresponding reaction-systems. We may speak of a group of reaction-systems which are functioning at any one time, *i.e.*, not inhibited, as a dominant complex of reaction-patterns, and the stimuli which call out any of these responses as being in the focus of *attention* or *awakeness*.

This exclusion of most stimuli from the focus of awakeness makes it possible to see how certain responses rather than

[16] 'Inhibition' is here used to apply to a specific reaction-pattern in contradistinction to 'baulking' and 'suppression' which have the connotation of preventing the achievement of some end aimed at, the means (presumably a response) not being specified uniquely.

others are called out, so that if the complex should include chiefly responses which happen to be more or less appropriate to bringing about a determinate result, the complex continuing to consist of these responses until the result occurs and then shifting, the behavior of the organism would properly be designated as telic—a series of responses each of which appears as a 'trial' and each but the necessary ones as an 'error.' The persistence of such a complex may be accounted for sometimes in terms of a persistent stimulus, or perhaps we should say simply antecedent condition which 'induces' the complex or tends to keep the same general set of responses inhibited. To be sure, such an inducing stimulus is not necessary to attention or awakeness, attention or awakeness being frequently a shifting affair including at each moment a group of response-patterns different from that which it included at the preceding moment. Indeed there seems to be no reason to suppose that it may not sometimes include a fairly constant group of response-patterns for a considerable interval, even in the absence of a continued inducing stimulus.

When a relatively permanent complex (usually maintained by a continued inducing stimulus) includes chiefly acts appropriate to bringing about a specifiable result, we may call it a 'drive,' as the general case of behavior which proceeds toward a result or end. Actually, the end is the elimination of the inducing stimulus, or perhaps the presentation of a new stimulus which directs attention or awakeness along other lines by inhibiting a new set of reaction-patterns and calling out a different set of responses. Thus the trail of a fox may be an inducing stimulus to a dog, inhibiting responses not appropriate to pursuing and catching the fox, while the catching of the fox would present a new set of stimuli, for which the corresponding reaction-patterns have not been inhibited in the pursuit complex. The presence of these new stimuli would shift the dog's attention inhibiting the pursuit responses. In other words, while the trail or scent does not inhibit those responses which may be called out by the actual presence of the fox, the sight, etc. of the fox does inhibit further follow-the-trail responses. We might say that the

dog 'desired' to catch the fox (had a pursue-the-fox drive), and that the catching of him 'satisfied the desire' (shifted pursuit responses out of the dominant complex). It would be quite in accord with this theory of drives that the dog should lose the trail of the fox and still keep on making pursuit responses, that is, responses appropriate to bringing about the result of catching the fox, the complex continuing in the absence of the inducing stimulus but no new stimulus of such a kind as to shift the attention being presented.

Such an hypothesis regarding drives will account in stimulus-response terms for two of the essential characteristics of telic behavior: (1) the continuation of a particular group of response-patterns in the dominant complex until the end or result (elimination of the inducing stimulus or presentation of a new inducing stimulus) is brought about and the focus of awakeness is shifted to new stimuli; and (2) the variety of responses that the organism makes while this stimulus continues.[17] It remains to account for (3) the inclusion in the complex of responses which are appropriate to bringing about the result on other grounds than their appropriateness as such. If we were to explain the inclusion of these responses in the complex on the basis of their appropriateness, we would still be giving a teleological account of telic behavior, an account which attributed the selection of antecedents (responses in the dominant complex) to a consequent (the result which they are appropriate to bring about). A partial non-teleological account of this inclusion of appropriate response-patterns may be made by an adaptation of a suggestion of Watson's—on previous occasions the shifting of the complex must have required some of these responses, so that they tend to occur on the whole more frequently [18] than inappropriate responses and so to continue in the complex. Once a complex (inhibition of a large group of reaction-

[17] *Cf.* on this formulation of the problem Tolman, E. C., 'Instinct and Purpose,' PSYCHOL. REV., 27, pp. 119 *ff.*, 217 *ff.*; Kantor, J. R., 'Principles of Psychology,' 1924, pp. 119 *ff.* *Cf.* also Warren, H. C., 'Mechanism and Teleology in Psychology,' PSYCHOL. REV., 32, pp. 266–284. Warren seems almost to identify the problem of teleology with that of the uniformity of nature.

[18] Watson, J. B., 'Psychology from the Standpoint of a Behaviorist,' 1919, p. 294.

patterns) induced by a particular condition has begun to develop, it will tend to continue to function until this condition is removed, and so responses which will remove it tend to occur each time the complex becomes dominant and to become conditioned to stimuli which commonly accompany the inducing stimulus. On the other hand responses not appropriate to eliminating the inducing stimulus need not occur if the complex is to cease dominance, and their less frequent occurrence makes it less likely that they will be conditioned to stimuli which commonly accompany the inducing stimulus. It may further be noted that some inappropriate responses may have become inhibited by conflict with appropriate response-patterns which are included. But this is admittedly only a partial accounting for the appropriateness of the responses included in the drive, so far as those responses in any particular case actually are appropriate, and certainly the appropriateness of many included responses is not very apparent.

The difference between the dog pursuing the fox and human purposive behavior might be described by saying that man is better equipped than the dog with inducing stimuli, which unlike the path, he can carry around with him. Even the dog can carry an empty stomach around with him. But man may carry as an inducing stimulus not only such conditions as an empty stomach and conditioned sensory responses (*e.g.*, internally aroused responses of the visual or auditory receptor-system, including the afferent nerves) but also language responses to substitutes for external sensory stimuli. It will be noted that 'conditioned sensory response' and 'language response' to internal stimuli which are substitutes for external sensory stimuli are behavioristic equivalents of the ambiguous introspective term 'image.' The significance of language responses as possible inducing stimuli in addition to conditioned sensory responses and body conditions such as an empty stomach lies partly in their great variety and their readiness of combination in countless concatenations, and partly in their symbolic or representative character. When the language response inducing a complex

of the drive type describes or represents the end-result toward which the drive proceeds, or when the 'original' stimuli to the pattern of a conditioned sensory response acting as such an inducing stimulus are elements of the *end* situation, we may speak of the behavior in this case as *purposive* and of the inducing stimulus (language response or conditioned sensory response) as the 'conscious' purpose.

According to this view of desires and purposes, the evolution of drives or desires is partly a process of adding new reaction-patterns to a given drive, through developing responses to new stimuli (association or conditioning), and partly a process of developing inhibitions to one of two mutually conflicting responses when both are called out together, because they happen to be present in the same dominant complex. Thus responses are added to and taken away from any given complex continually. Moreover, it is to be remembered that the individual reaction-systems of which a complex is made up, are themselves complexes or congeries of the potential functioning of many muscles, glands, and nerves—are behavior units only in that these several structures tend to function simultaneously or in concatenation according to a recurring pattern. In so far as these individual patterns are altered by 'experience' or the history of the organism, the drives into which they are organized are subject to this more fundamental type of change. A drive, then, as a complex of reaction-systems or group of reaction-patterns which remain uninhibited at the same time is subject to alteration either by the addition of new reaction-systems to the complex or by the elimination of old ones from it. It is also capable of modification by changes in the particular group of glands, muscles or nerves whose functioning constitutes a reaction-pattern which is a part of the drive.

IV

So much for the attempt to sketch in non-teleological terms an account of desire and purpose. It remains to consider another aspect of telic behavior,—choice. Viewed teleologically, choice has commonly been conceived as arising

out of a conflict between two ends (consequents), each of which would, but for the other, determine (antecedent) behavior, and as a process of resolution (or arbitration?) of this conflict through which one of the ends comes to determine behavior. So long as desire and purpose are conceived teleologically, *i.e.*, as determining ends or tendencies, choice may be regarded as a resolution of the conflict between two given desires or purposes. It is tempting on such a view to regard the desires (or purposes) as of given strength or intensity, so that choice becomes the survival of the more intense desire. Moreover, so far as the choice itself goes, according to this theory, there is nothing to prevent any given conflict and survival from recurring again and again. Each time the conflict occurs, the greater strength or intensity of one desire insures its displacing the other as a determiner of action. The choice has to be made over again each time the two alternatives are open.[19]

But if the present view of desire and purpose is accepted, we cannot conceive choice as the resolving of a conflict of drives (dominant complexes), since in the nature of the case there is but one complex dominant at any one time. As a matter of *objective description* there is no basis for dividing the complex into two parts (although *functionally* some of the reaction-patterns in the complex may be appraised as subserving one end and some as subserving another and even conflicting end). The complex simply consists of those reaction-patterns which are not subject to inhibition at the time—not 'dissociated' as some psychiatrists would say.[20] The 'conflict,' therefore, is presumably between two reaction-patterns which are simultaneously stimulated but which

[19] It is possible on such a theory, since it evades the task of putting desire into stimulus-response terms, to suppose that these repetitions of choice are of an increasingly elliptical character, analogous to repetitions in motor learning. The chief objection to this view of choice then becomes the difficulty of putting it into stimulus-response terms.

[20] 'Dissociation' and 'association' are not used as antonyms in this paper. 'Dissociation from a complex' means 'inhibition by the inducing stimulus.' Two reaction-patterns do not become parts of the same complex by being 'associated.' 'Association of one reaction-pattern with another' means that the second becomes a substitute stimulus appropriate to calling out the first.

cannot function together, either perhaps inhibiting the functioning of the other. Thus choice becomes the process of development of inhibition of one of these responses, the other being allowed to function. In other words choice is a process of the elimination of a reaction-pattern from the dominant complex. We do not have two desires of given strength in conflict, but rather one complex containing two mutually inhibitory reaction-patterns which have been stimulated together—these reactions possibly appraised as subserving conflicting ends. And choice is a process of changing this complex, is part of the evolution of the complex (which may be a desire- or purpose-drive). Moreover, once choice has occurred (*i.e.*, once inhibition has been developed) the personality is modified so that a repetition is not involved, though the choice situation may have to occur more than once before the inhibition is thoroughly developed.

V

If it is granted that desire, choice, and purpose as phases of telic behavior can be described from a natural-evolutionary standpoint in non-teleological terms such as reaction-pattern, inhibition, and association, and that so describing them in no wise destroys their telic character, it may be worth while to consider briefly some of the conclusions which the present attempt at a non-teleological account suggests for related problems. The problems on which it is proposed to discuss the bearing of the above theory are rather diverse, which imposes on us the somewhat ungraceful necessity of jumping rapidly from one field to another. (1) We may consider first the marginal utility theory of *choice* which has been almost universally employed by economists in explaining price-offers and offers of goods at a price. (2) In this connection it will be interesting for historical reasons to note a possible bearing of the above theory on the relationship between pleasure and pain. (3) We may then consider briefly the attempts by so-called 'social psychologists' to account for culture in terms of human functional heredity.

1. An inherited division of scientific labor has left the economist in the rather awkward position of having to develop a psychology of choice somewhat apart from the main current of psychological thought. Without attempting a detailed discussion of this psychology of choice, commonly called the Marginal Utility Theory, we may say that it treats desires, or more accurately, desirednesses (utilities) as given teleological magnitudes or quantities not changed by choice, the origin of which the economist need not investigate.[21] The price offered for an article according to the more accurate formulations of this view is a ratio of two 'marginal' desirednesses.[22]

Clearly such a theory of choice is in conflict with an attempt at a natural-evolutionary account like that outlined above. If choice is a process of alteration of complexes, including drives or desires, it will not do to regard desires as given magnitudes unaltered by choice or to neglect their origins. Moreover, the relation between price-offer, as the result of a choice or change in desire, and utility as a state of being desired to a given 'extent' is not, as many economists have supposed, one between a quantity of utility (desiredness) and a ratio between two such quantities. Rather price-offer is one of the responses of which the desire consists; and choice is a process of changing desires, as a result of which there is not a relation between two unaltered desires but rather one changed desire. Price is not a relation between two desires, since desire itself is a relative; desire is a desire for one thing rather than others and so in effect a completed

[21] *Cf.* the following passages: "Whether a desire or want traces back to instinct or to impulse or to experience, is an inquiry not as to the existence of the want but as to the genesis of the want. This is not especially the economist's task. . . . It is enough for the economist that the desire exists, that the external thing attracts; thereby it is a good in the mere sense that it is desired; one wants it."

"Desires conflict with one another and defeat one another of satisfaction. The individual has a limited purchasing power . . . and hence must make a choice of what he will buy. He has to decide, and his decision, as he knows, is really the fulfilling of one desire on terms of thwarting another. . . . The utility or desiredness analysis is nothing more than a schematic and very abstract account of this process of making these choices." Davenport, H. J., 'Economics of Enterprise,' 1919, pp. 99–101.

[22] *Cf.* Fisher, I., 'Elementary Principles of Economics,' 1912, p. 289.

choice. Desiredness is preferredness. Choice does not take place between two desirednesses, since what is desired is changing during the choice-process; choice is rather a conflict between two reaction-patterns and a process of survival of one of them in the complex.

Moreover, there is nothing in such a theory of desire and choice to show why response *A* should not inhibit response *B*, in one situation, response *B* inhibit response *C* in another, and response *C* inhibit response *A* in a third situation. Yet the marginal utility theory of choice seems to require just this (in assuming that desires differ from each other quantitatively) [23] so far as choice is 'rational' in the sense of choosing that which one desires 'most.' If desire *A* takes precedence over (is more intense than) desire *B*, and if *B* takes precedence over (is stronger than) *C*, it would seem to follow (from this assumption of the quantitative nature of desire) that desire *A* would take precedence over (be stronger than) *C*.[23a] In other words, preference is assumed by this view to be what has been called a 'transitive' relation. On this point also there is a possible conflict between this assumption of transitiveness which is necessary to the marginal utility theory, and the present position on the nature of drives and their development, according to which there seems to be no reason to suppose that behavior is 'rational' in the sense that preference is a 'transitive' relation, that preferring *x* to *y*, and *y* to *z* involves preferring *x* to *z*.

2. The theory of desire and choice outlined above offers an interesting possibility for the theory of pleasure and pain. It allows pleasure to be understood as a process of accomplishment of a desired result, while pain may be thought of as a process of developing inhibition of the responses involved in most desires. Since pain nerve impulses commonly take precedence over others in the nervous system, the stimulation

[23] This assumption is also implied in supposing desires to differ in strength or intensity. For a recent illustration of this assumption by a psychologist, see Moss, F. A., 'Study of Animal Drives,' *J. Exper. Psychol.*, 1924, 7, pp. 165–185.

[23a] Note that this point involves the preceding one: a shift from 'one response takes precedence over another' to 'one desire takes precedence over another.'

of pain receptors tends to involve the development of inhibition of responses to stimuli which affect the other sensory receptors. Thus whatever we are doing, whatever our desires and purposes, a pain stimulus is likely to lead to inhibition of our action. Speaking popularly we would say that pain prevents the 'satisfaction of most desires.' Thus there is an opposition between pleasure as the process of accomplishment, and pain as the process of prevention of accomplishment, which bears out the common-sense notion on the subject, in spite of the fact that we have specific pain receptor organs but not apparently specific pleasure receptor organs.

Somewhat similarly when stimuli to mutually conflicting responses give rise to a process of developing inhibition (*i.e.*, require a choice) the situation may be thought of as unpleasant, so that 'unpleasantness' is in a certain sense also opposed to pleasure.

3. Those social scientists who have employed 'social psychology' in their theories, have usually attempted to use some theory of human functional heredity to explain the organization or functioning of social groups. If we discard the teleological conception of instinct, what bearing has this on social theory, and what kind of a view of functional heredity is one to hold?

The present theory of telic behavior formulated in non-teleological or stimulus-and-response terms allows for the possibility of inheriting, or at any rate of having at birth certain drives. But presumably these drives are all capable of undergoing and necessarily do undergo vast development as the organism continues life. Consequently it becomes necessary to understand behavior largely in terms of habit or 'second nature' and we have to 'explain' behavior by 'explaining' the development of various reaction-patterns and the ways in which they are organized into complexes.

In order to avoid certain misunderstandings on the part of those who have advocated instinct (or intelligence) in the teleological sense as an hereditary trait and as an ultimate psychological category, it may be well to emphasize the fact that such a position as is here outlined does not mean that

one begins life with a *tabula rasa*, nor does it mean that hereditary nature does not determine in part the sorts of 'associations' or habits that one may acquire. At birth one presumably has (*a*) reaction-patterns, some relatively unmodifiable except as to the stimulus which may call them out (reflexes) and some relatively modifiable ('instincts'), and (*b*) inhibitions, perhaps already organizing the reaction-patterns into some more or less definite complexes. Moreover, (*c*), one's later behavior is by heredity structurally limited in respect to (1) the stimuli to which it is sensitive; (2) the types of response (*e.g.*, some postures are impossible) and the physical power and speed of response, etc.; and presumably (3) the ability to 'learn,' or develop a particular pattern of neural, glandular, and muscular functioning as a response to a given stimulus. These limitations or 'capacities' are important factors in accounting both for individual differences among men and for differences between men and the more closely related mammals. And capacity or limitation, be it noted, is, unlike the teleological conception of instinct for which it is intended as a partial substitute, a non-teleological category.

In such a view of human nature it still remains to account for the fact that in the same culture different individuals have somewhat similar desires and purposes and habits. It still remains to describe how on the one hand the child is introduced into an existing culture, how the standards of taste and common interests of the group are inculcated in him under any given cultural situation; and on the other hand, how the set of tastes and desires common to the group came to be as it is, as a matter not of individual but of group history. In other words, there are two problems: the ontogeny and the phylogeny of institutions and mores.[24] Values, or 'desirednesses' which have validity for a group of people according to such a view, are therefore to be accounted for ontogenetically in terms of social heredity or education; the individual is educated to accept the standards of his group.

[24] *Cf.* Ayres, C. E., 'Instinct and Capacity,' *J. Phil.*, 1921, **18**, pp. 600–606.

According to this view, then, it will not do to account for social organization by attributing it to a gregarious instinct, or property, by attributing it to an instinct of self-assertion. The job of the social scientist is vastly more difficult than this, and more specific. He will have to study the specific mores and taboos of which social organization or property rights consist in any culture, and investigate the history of these prevalent types of behavior—how they came to survive in the process of cultural evolution. Moreover, he will need to investigate the processes by which typically a child is 'brought up' so as to acquire the drives and habits common to most adults, of which in the aggregate our social organization and our institutions consist.

To summarize: The present view attempts to account for telic behavior according to a natural selection rather than a supernatural selection hypothesis, *i.e.*, one in which selection of responses is by antecedent stimuli rather than by subsequent ends. But it is possible and desirable on the present view to specify the ends which certain acts subserve as functions, giving a basis of appraisal of the behavior of the organism. Furthermore, the present view allows for an extreme variableness of specific desires and purposes as between individuals and perhaps an even greater variety of specific reaction-patterns than of drives, if the drives are named for their 'ends.' It also allows for the variation of an individual's specific desires and purposes from time to time. Choice appears not as a comparison of desirednesses, whose development need not be investigated, but as a process of development of desire during which desiredness is in process of change. Moreover, it is probably an over-rationalization of behavior to assume that because one prefers *A* to *B*, and *B* to *C*, one desires *A* 'more than' *C*. Again, the present view requires a careful distinction between the ontogenetic analysis or account of the process by which an individual comes historically to get his reaction-patterns and complexes, and the phylogenetic analysis or historical account of the process by which a given set of habits and complexes comes to be typical among a

particular group of people at a particular period. A natural-evolutionary psychology cannot furnish the social scientist with an easy, ready-made solution to the problems of group behavior. For the most part, apparently, he must solve these problems himself having recourse to the slow method of a painstaking investigation of social facts, to an historical and comparative study of group behavior.

THE MENTAL AGE CONCEPT

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It is the purpose of this article to show that the mental age concept is a failure in that it leads to ambiguities and inconsistencies. In discussing the logic of the mental age concept, we shall have occasion to describe two definitions of mental age, both of which are in use and both of which lead to the same ambiguities.

The two definitions of mental age which are either stated or implied in current mental test work are as follows:

1. The mental age corresponding to any given test performance is that chronological age for which the test performance is the average.

2. The mental age corresponding to any given test performance is the average chronological age of people who make that test performance.

These two definitions of mental age do not have the same numerical values. Since they are inconsistent, it is essential, if we insist on using mental ages, that we specify rather definitely which of the two possible kinds of mental age we are using. They are not interchangeable. Both of them may be defended, but the first definition is the one usually followed.

In order to set out in relief the difference between these two definitions of mental age, we may recall the meaning of the two regression lines on a correlation table and their application in this case. When you step on the scales at the drug store, you notice a little table with two columns, heights and weights. You ordinarily read it by saying that your height is so and so. You find this height in the table, and next to that entry you read the average weight of people with your height. You have used the record of a regression

line. But now suppose that when you have ascertained your weight, you try to find the average *height* of people who have your *weight*. Your impulse is to find your weight in the same table and to read the height next to it. But that doesn't work. Another table would have to be used which would be constructed from another regression line. One of these tables shows the average *weight* of people with given *heights*. The other table would show the average *height* of people with given *weights*. It is the former table in which we are ordinarily interested.

Now, if the same reasoning is applied to a correlation table for test performance and chronological age, we shall have two regression lines here also. One of these lines, or tables, will show the average test performance for children of given age. The other line, or table, will show the average chronological age for children of given test performance. But these two lines are different and the two tables cannot be used interchangeably. Their practical use would be illustrated as follows. If the age of the child is known and we want to know the average or expected test performance, we use the regression 'test performance on age.' If, on the other hand, the test performance is known and we want to guess the child's chronological age, we should use the regression 'age on test performance.' When we test children's intelligence, we nearly always have access to their chronological age. We don't need to guess at that. But we need to do a lot of guessing about the child's intelligence. Therefore one of these regressions is of considerable interest and practical importance whereas the other one is of practically no importance. The important regression line in this correlation table is therefore the regression of 'test performance on age.' It can be used to ascertain the test performance that is to be expected of the average child of a given or known age.

Figure 1 will assist in making clear this distinction. Let it represent the correlation table for test performance and chronological age. Let test performance be measured in any terms or units. In the vertical column, x, are represented all the children of age x who took the test, and their test

performances are indicated by the dots in that column. The average test performance of all these children of age x is indicated at A. If a similar column is erected at each age we should of course find that the older children have higher

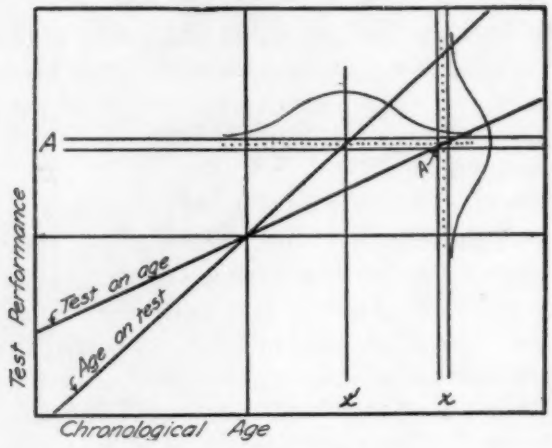

FIG. 1

average test performances than the younger children. If the average test performance is located in each column, these average performances will constitute the regression 'test on age.' By means of this regression line we can ascertain the average or expected test performance for a child of any given age within the limits of the table. In fact, by the more customary definition of mental age, each of these average test performances is designated as the mental age of the column in which it is found. Therefore, the test score, or performance A is designated as the mental age x because A is the average test performance or score for children of age x. This is the first of our definitions of mental age and it is the customary one.

But in the same table it is also possible to draw another regression line. Suppose that we have collected all the test records with the score A. All of these children will be represented in the correlation table of Fig. 1 in the horizontal row at the level A. Each child is represented in such a table by a dot or other suitable mark. Now we may determine the

average chronological age of all the children who get that particular score *A*. Let that average age be represented by *x'* in Fig. 1. We may of course do likewise for every class interval of test performance, and that will give us a set of horizontal rows, each with its own average age. It is to be expected that as we increase in test performance, the average chronological age will also increase. If these average ages of the successive horizontal rows be connected, we shall have the regression line 'age on test.'

What is the significance of these two regression lines in our definition of mental age? Let us attempt to define just what we mean by a mental age of eight. If we have tabulated our data for age and test score in the form of a correlation table like Fig. 1, we shall have two alternatives which refer to the two regression lines. Suppose that the *x*-column represents the eight-year old children and their test scores. We can then determine the average test score for these eight-year old children. We may designate that average test score, *A*, as the normal performance for eight-year old children and we may argue that this test performance should therefore be called the mental age of eight. When a child attains this test performance, *A*, we should say that the child has a mental age of eight.

But there is another consideration that makes this interpretation look awkward, although it is the customary one. Suppose that we consider in one group all the children who have this test performance, *A*. What is their average chronological age? It is certainly not eight unless there is a perfect correlation between chronological age and test performance, which is an impossibility. We therefore find that the average test performance of eight-year olds is not the same as that test performance, the average age for which is eight. We may, however, select one of these analyses as basic for a definition of mental age. We might say, for example, that a certain test performance is to be regarded as a mental age of eight if the average age of all the people who get that score is eight. That is the second definition of mental age. According to this definition the test per-

formance A would be assigned a mental age at x' in Fig. 1.

In the practical situation we might proceed along either of these two lines. When a child makes a certain test score, A, we may ask the question, "What is the average chronological age of other children who make this particular test score, A"? In Fig. 1 this age is designated x'. On the other hand, we may ask about that same child the question, "What is the chronological age, the average test score for which is equal to that of this particular child"? Then, if the child made a test score of A, we should find the answer at x and not at x'. This latter definition is the one commonly implied by the term mental age, but the usage is not consistent and investigators may possibly be unaware of this difference between their mental age norms.

There is another aspect of the logic of the mental age concept which goes contrary to the common sense of the correlation table. In the height-weight example, we found that when the height is known and we wish to ascertain the average weight corresponding to our height, we use the regression 'weight on height.' When the weight is known, and we wish to ascertain the average height for our weight, we use the regression 'height on weight.' This can be summarized by the rule that we always use the regression 'unknown on known.' That is not only common practice but it is also common sense in the use of the correlation table.

But when this reasoning is applied to the correlation table for test score and chronological age, it leads to the less popular definition of mental age. When a child has attained a certain test score, it is the test score that is known and if any estimate is to be based on the test score, we should be estimating the chronological age by the test score. In other words, we should be using the regression 'age on score.' We should then define the mental age of a child as the average chronological age of all children who make the test score of this particular child. According to this definition, a child would be at par mentally if its test performance is such that the average age of all people who make that test score is equal

to the child's age. That is our second and less popular definition of mental age.

So far we have considered some of the inconsistencies which are the result of using two definitions of mental age. However, either one of these definitions might be adopted and universally used so that we should always know which is which. Some other name might be adopted for the other definition if it were found advisable to use both measures of brightness or test intelligence. My main argument is, however, that both of these definitions of mental age lead to ambiguities when applied to the adult years, and that the mental age concept should therefore be discarded in favor of a more direct and simpler measure of brightness which does not lead to logical somersaults like those of mental age.

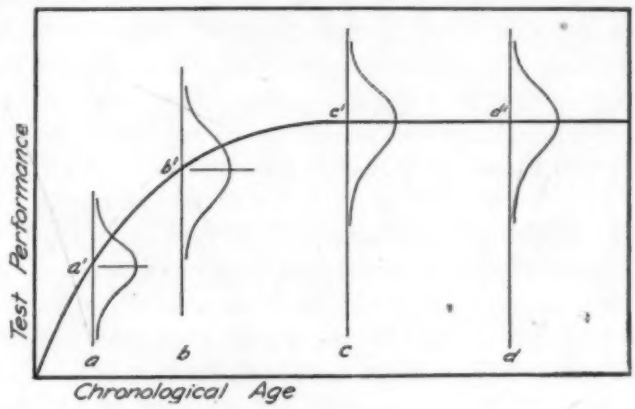

FIG. 2

In Fig. 2 I have represented schematically the same correlation table as in Fig. 1, except that the age range has been extended into the adult years. It represents an analysis of the first definition of mental age as it appears in the adult ages. At *a* we have the distribution of test performance for age *a*. The average test performance at that age is at *a'* and the distribution of test performance ranges above and below the performance *a'* as a mean. For our present purpose the question of the normality or symmetry of this distribution does not matter. At a higher age, *b*, we have

similarly a distribution of test performances which range above and below the mean test performance at b'. Naturally we should expect the mean performance b' to be higher than the mean performance at a', because b represents a higher chronological age than a. At the age c which is somewhere in the teens, or perhaps as high as twenty, we have the maximum test performance which our mental age tests can differentiate. The highest mean test performance, c', is attained at the age c. But now the difficulty arises in applying the mental age concept. If we inspect the distribution of test performance at the adult age d, which may be age 40, for example, we find that its mean test performance d' is no higher than the mean test performance already attained at the age c. If the age c, at which the maximum mean test performance is attained, is found experimentally to be at 16, for example, then it will automatically be impossible to attain a mental age beyond that age. The reason may be in the limitations of our tests which do not measure mental development beyond adolescence, or the reason may be in the possible conclusion that intelligence does not develop beyond that age. At any rate we must deal with the fact that a group of 40-year olds would make a mean test performance which would be no higher than the mean test performance at 16. Now let us look at the first definition of mental age which we are here trying to interpret. It states that the mental age corresponding to any given test performance is that chronological age for which the test performance is the average. It will readily be seen that since the mean test performances c' and d' are equal, we shall have an indefinite number of mental ages assigned to this particular test performance. We have a right to say that the test performance c' should be given a mental age of 40 because a group of 40-year olds would make a mean test performance of c'. But a group of 16-year olds would also make this mean test performance. Therefore this definition of mental age is entirely ambiguous. A mental age of 40 is the same as a mental age of 16 or of any other age beyond the 16- or 18-year level. The only way out is to revise the definition of mental age.

This difficulty is well known to all mental test people. The solution usually given is arbitrarily to call the mean test performance of 16- or 18-year old children the adult level. Walter Lippman in his articles in the *New Republic* a few years ago, drove home rather effectively the absurdity of insisting that the adult population has an average mental age of 12 or 13 by pointing out that the Army test data constituted as valid an age standardization as the few hundred tests given to school children. We might be able to save the mental age term by revising the definition so that the mental age corresponding to any given test performance is *the lowest* chronological age for which the test performance is the average. But why use age at all as a scale for mental development when the relation between intelligence and age is imperfect and non-linear? Why not specify test performance on any convenient scale directly related to the test performance itself, instead of twisting it into some sort of age?

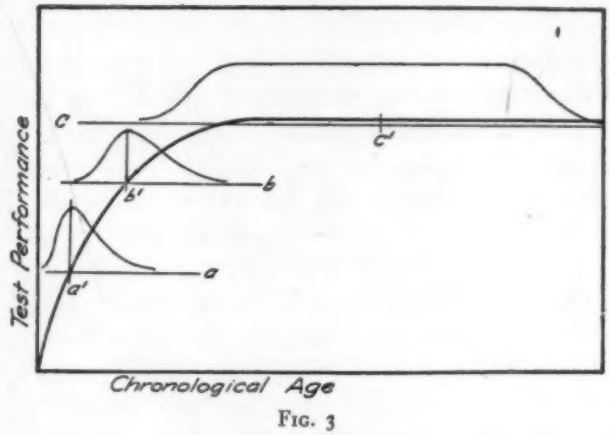

FIG. 3

But there remains the second alternative definition of mental age which may be interpreted for the adult years. In Fig. 3 I have represented again the correlation table for test performance and chronological age. In this figure the horizontal section *a* represents all the people who have the test performance *a*. They vary in age, of course, and the distribution of age is represented diagrammatically by the

frequency distribution with its mean a'. In the same manner the horizontal section b represents all the subjects whose test performance is b and their ages are diagrammatically represented by the frequency distribution with its mean at b'. But what about the distribution of ages for the test performance c? Since this is the mean of the adult test performance the age distribution stretches over the whole range of life. The mean age c' would be perhaps 35 or higher. Even if we should decide to determine this mean age, we should find it impossible to do so without ambiguity because the mean age for this distribution, and for those immediately below it, is largely determined by the number of individuals that we should select at each age. Should we select the same number of people at each age in this distribution? Or should we select such a frequency of individuals at each age that the distribution of age corresponds to the frequency of each age in the general population? Such a distribution might be determined from the mortality tables. Since the mental age scale is intended as a scale of mental development, there is no sense in translating test performance into age with all these entirely unnecessary troubles. In the case of the first alternative definition of mental age, we found that the term could be saved from absurdity, if we must have it, by redefining it. But in the nature of the case, there is probably no way in which the second alternative meaning of the term may be redefined so as to avoid ambiguity.

All of these difficulties would be avoided entirely if we should discard the awkward mental age concept and proceed instead as follows. When a child has been given a mental examination of the Binet type, or any other, we might ascertain the child's chronological age. Then we might consult our norms and say that this child's mental development, compared with other children of the same age, has a percentile standing of, say, 70. This would be extremely simple. It would mean that this child is a little above the average in intelligence among children of its own age. It would mean that 30 per cent of the children of the same age are brighter, and that 70 per cent are less bright. The

intelligence quotient would be discarded along with the mental age concept on which it depends and the percentile standing of the child would be used instead. Such questions as the constancy of the intelligence quotient would still be with us but we should use the percentile standing of the child among others of its own age as the index, instead of the intelligence quotient. We should be inquiring whether a child keeps his relative standing among other children of the same age as he grows older.

For many statistical studies it might be preferable to use the sigma standing of the child instead of the percentile standing. The test performance of any particular child might then be designated either by his percentile standing or by his sigma standing, in every case compared with other children of the same age. If such a system were in use, we should have no difficulty whatever at the adult level. We should measure mentality there also by stating the person's percentile standing, or his sigma standing, compared with other adults or with others of the same age. Our measurement system would have continuity and sense, a satisfaction that cannot be attained with mental ages and intelligence quotients.

If we should measure mental development in terms of relative standing, either in terms of percentile ranks or the standard deviation, it would still be possible to say that this particular child of eight is as bright as the average ten-year old child. That is the comparison that the mental age terminology is intended to facilitate, but there is nothing to prevent our making such statements wherever wanted without using mental ages. In practical situations, both types of comparison may be made, one directly with other children of the same age and one comparison with children of any other given age. Then a given eight-year old child's mentality would be described by a percentile of, say, 70 for his own age, and he could also be compared with ten-year old children by saying that he would have a percentile of, say, 50 among them and so on. The child could be compared with children of other ages by the same set of tables or norms.

Binet may still be given credit for having introduced certain types of objectivity in mental measurement but his invention of the mental age concept was an awkward and unfortunate one. It is of course possible to retain the terminology of mental ages and intelligence quotients and to limit these terms to tests for children. But since these terms become nonsense when applied to adults, there would always be the question as to what age shall be the practical limit for their use. Would it not be better to discard mental ages and intelligence quotients and to adopt a measuring system, already universally known, which is extremely simple and which has the advantage of continuity so that it may be used for all ages?

THINKING AS AN INSTINCT

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Perhaps the most persistent trend in modern psychology is its tendency to shift the emphasis from intellectual to non-intellectual factors in its explanations. The 'rational man' of former years has almost disappeared from the psychological scene. We no longer ask, when we wish to understand a bit of conduct, what reasons actuated it, but what biological stimuli initiated it.

On the whole, this movement represents one of the most promising turns that psychological thought has taken. Incidentally, however, it has left the intellectual processes themselves in a somewhat ambiguous situation. If man can no longer be understood as the reasoning animal, what are we to believe of the alleged processes of reasoning? May not their very existence be a gratuitous assumption to account for erroneous impressions we have formed of human behavior?

This attitude most commonly finds expression not in direct questions, but in a tendency to regard with more than a trace of suspicion any attempt to introduce so-called intellectual factors into psychological explanations. It has come out rather prominently in two fairly distinct lines of criticism.

The first consists in showing that the activities usually called thinking are really something else. It is made by those psychologists who are interested in demonstrating that the intellectual processes are nothing other than the humble trial-and-error procedures which can be observed in animal learning,—a mere matter of language mechanisms or other receptor-connector-effector processes. Their aim is to take the mystery out of thinking by reducing it to simpler and presumably better understood processes, usually of the reflex or conditioned reflex type. To them, the ghost of animism lurks behind such terms as 'intellect,' 'thought,' and 'reason,'

and the way to lay that ghost is to show that the activities called by those names are merely complex integrations of simple processes which can be mechanistically conceived.

The second line of attack is made by the psychoanalysts. They are so well acquainted with the phenomena of rationalization that they regard most reasons with suspicion. Their attitude toward reasoning is not that it does not exist, but that its rôle is relatively insignificant. On its own account, it does nothing. It is not the master of human conduct, but a servant of the instincts—and a corruptible servant at that. Only the psychologically unsophisticated will seek the source of an action in its reasons.

The value of this sceptical attitude toward the intellect can hardly be overestimated. It has made possible a deeper penetration into human motivation, has divested our notions of human nature of much of their artificiality, and has brought some of the most intricate of psychological processes out of the field of the hopelessly inexplicable. Yet it cannot be denied that this summary treatment of the activities which have been called thinking raises some difficulties of its own.

The psychoanalysts in particular become involved in a curious confusion. If our intellectual organization makes no claims of its own, why do we bother to rationalize? Why do we not follow our instincts without extenuation? The fact that we do not, suggests that if we are to be thoroughly comfortable, we must satisfy the intellectual as well as the instinctive demands of the situation. It is not enough to give an instinct its satisfaction; there must be a solution of the problem as well. Neither demand will subside completely and rationalization is the result. The tendency to rationalize is usually taken as an indication of the strength of the thwarted instinct—of the fact that it will come out in one form if not in another. But it is just as indicative of a tendency to make a situation 'come out right' by thinking about it, by manipulating the elements of the problem until some kind of adjustment is achieved, though in rationalization it is only a sham adjustment, just as the satisfaction of the

instinct involved is often only a sham satisfaction. Furthermore, the very goal of psychoanalysis as a therapeutic measure is insight, an intellectual envisagement of one's predicament. Moreover, the insight *constitutes* the cure. The understanding of the complex is itself the resolution of the conflict and the correction of conduct is its consequence. The whole situation makes somewhat obscure the slave-master relation of intellect and instinct.

Those psychologists who dismiss thinking by reducing it to simpler processes are not guilty of confusion but of ignoring part of the problem. It is undoubtedly important in understanding a given activity to see how it resembles known processes and how it may be analyzed into them. But it is also important to notice the specific and distinguishing features of the activity as a whole—the very characteristics which singled it out in the first place as an entity sufficiently definite to constitute a distinct object of study. Köhler [1] says on this point:

"There is probably no association psychologist who does not in his own unprejudiced observations, distinguish, and, to a certain extent, contrast unintelligent behavior and intelligent. For what is association psychology other than the theory that one can trace back to the phenomena of a generally-known simple association type even occurrences which, according to unbiassed observation, do not at first make the impression of being identical, above all, so-called intelligence performances? In short, it is just these differences which are the starting point of a strict association psychology; it is they which need to be theoretically accounted for."

As long as psychology ignores the special characteristics of thinking as an identifiable unit of behavior, it ignores some of the very conditions that give the problem its being, and there is some danger that this half of the case may be elided in a preoccupation with the other and equally important half, its similarities to other kinds of behavior.

[1] Köhler, W., 'The Mentality of Apes.' (Translated by Ella Winter.) New York, Harcourt, Brace and Co., 1925, pp. 2 and 3.

The root of the difficulty seems to lie in assuming a contrast between the intellectual and biological functions of the organism; or rather—since that assumption, if explicitly stated, would probably be denied—in the fact that the terms 'intellectual' and 'biological' have acquired connotations which make them seem incompatible. One reason why many psychologists do not like to deal with 'intellectual' material is that it seems to them to involve the danger of becoming 'dualistic' or 'traditional' or 'mentalistic,' to imply a return to a metaphysical as contrasted with a scientific manner of treatment. And one reason why the psychoanalysts have placed so much emphasis on the instinctive and unrational nature of human drives is that they have had to combat the artificial notion of intellect implied in the old conception of the rational man. But it is due to historical accident rather than to the necessities of the case that thinking has been treated from the metaphysical rather than from the scientific standpoint. It is entirely possible to conceive thinking in a thoroughly naturalistic way, and yet to do justice to both the specificity and the effectiveness of the activity.

It has, in fact, been so conceived. Dewey, perhaps more definitely than anyone else, has transplanted the process from the realm of pure logic to the soil of biology. His 'Essays in Logical Theory' and his 'How We Think' both reveal his habit of thinking of man "as an animal species finding his way about by the unique methods of the central nervous system on an insignificant planet with a very bad and variable climate." [2] James, too, holds similar views. His pragmatism, his 'will to believe,' and his discussion of conception in 'The Principles of Psychology' all make thinking an activity rooted in the needs of the animal organism and having as its function the creation of such 'truth' as will stand the wear and tear of human practice. Graham Wallas [3] points out that no line can be drawn between 'intelligent' and

[2] Ayres, C. E., 'Philosophy au Naturel,' *The New Republic*, March 25, 1925, pp. 129–131.

[3] Wallas, G., 'The Great Society,' New York, The Macmillan Co., 1917, pp. 32–56.

'instinctive' dispositions. His treatment plainly suggests that thinking may be regarded as an instinct.

It is the purpose of this paper to follow this lead and to show that psychology may profitably and properly classify thinking with the instincts. However, it must be noted at once that the whole question of whether or not there are any instincts, whether instinct is a useful concept for psychology generally, is irrelevant in this connection. The point to be made is that thinking is instinctive only as the so-called instincts are, not in any other sense; and since the instincts are safely within the realm of natural science, thinking, if it conforms to their general pattern, may be accepted as one of their company.

The advantages of regarding thinking as an instinct are mainly two. First, it will emphasize a point theoretically admitted but often practically ignored that thinking is quite as 'natural' a process as any other organic activity. It will help to establish it in a field where it may be investigated in all its specificity without fear that the investigator will encounter material which is not suitable to scientific treatment or will involve him in metaphysical presuppositions as to its ultimate nature. Second, thinking will be endowed with the same kind of 'drive' as any other biological process. The source of instinctive and intellectual activity will be recognized as the same, and will be found in whatever it is that makes protoplasm squirm. The question of how thought can influence instinct and *vice versa* will then become part of the general question of how one instinct, or any other biological process, can at times become dominant over another.

It is extremely difficult to give a satisfactory definition of instinct, especially as the concept has undergone so much adverse criticism recently. We shall begin, therefore, by using the term in an admittedly loose sense, attempting to include in it what has usually been meant by it in the literature. In the remainder of the paper, we shall try to show that thinking possesses the characteristics which are regarded as criteria of instinct in the generally accepted sense.

I. Perhaps the most fundamental point to be considered is that thinking is, at basis, a non-rational impulse. In his famous passage about the hen and her 'never-to-be-too-much-sat-upon' nestful of eggs, James has emphasized the fact that instinctive reactions are performed not because they have utility or any other external justification, but because they are intrinsically satisfying,[4]—because to every creature 'its ways' are so obvious and natural that the need of explaining them does not arise. But we commonly overlook the fact that human thinking is pursued in the same unquestioning way; that its reason for being is taken for granted; that for the most part we do not even ask whether it rests on a justifiable basis.

One of the clearest indications that this is the case is found in our everyday attitude toward causes and reasons. It seems so natural to want to know *why* a thing is so, that it does not occur to most of us to doubt that there *are* causes and reasons to be found. To many persons it is a strained and artificial conception that there may be nothing outside our own natures to justify our 'rational' procedures. Even if we cannot find a cause, we assume that there is one; in many cases we accept an unsatisfactory cause rather than do without one altogether. We feel uncomfortable and insecure without a cause, at least without the possibility of one. Furthermore, most people want a *final* cause, an ultimate reason for everything. To stop short of this goal gives a painful sense of frustration; to go headlong, to make a plunge for the ultimate solution is the primitive and natural procedure. To live without final causes for long periods of time is a feat of intellectual asceticism which most human beings find it difficult to maintain. From the standpoint of *comfort*, even intellectual comfort, it is better to accept a final cause of some sort, however vague and unsatisfactory, than to believe that there may be none, or to entertain the possibility that the whole 'rational' way of looking at things may have no justification outside the fact that it is a common human method of functioning.

[4] James, W., 'Principles of Psychology,' 2, pp. 386–388.

Logic itself contributes interesting evidence on the essentially non-rational basis of reasoning. When we come down to the ultimate necessities of our thinking, we find no reason why they should be as they are. We only know that, when brought to the point, we cannot think otherwise. It has been suggested that these fundamental logical principles may be due to very deeply embedded laws of our biological constitution, that they may be the most fundamental and general of our reactions, and therefore finally inescapable. This suggestion is itself a particularly good illustration of our inability to deny ourselves causes, but whether it is accepted or not, it leaves the compelling rightness of the fundamental logical necessities *logically* inexplicable.

Of course thinking may have an external justification in the same sense that other biological activities do. It may be useful to the continuance of human life, just as eating, reproducing, fighting, and fleeing are. But this only pushes the matter one step farther back; there is no reason outside of human preferences why human life should endure. Besides, the drive or internal justification of thinking is a different matter. We neither eat nor think to save our lives, but because we cannot help it.

So much has been said about thinking as a painful and difficult process that this point will probably seem forced. It is true that thinking is often attended by pain and difficulty, but this is true of every instinct so long as it is thwarted; and thinking, which is even more 'imperfect' than most of the instincts, rarely goes straight to the goal but involves false steps which necessarily bring checks and frustrations. The fact that it proceeds in spite of the pain it involves is indicative of the strength of its drive. Besides, the solution, which corresponds to the consummation of an instinct, is itself unquestionably satisfying, and the process itself, so long as it seems promising, involves a certain pleasurable excitement.

It may be objected, too, that if there is an instinctive drive to think, the activity should not be so infrequent and half-hearted as it is. But instinctive drives do not necessarily operate violently and incessantly; we do not expect an animal

to eat voraciously all the time. Besides, the view that it is extremely difficult to make a person think has probably been overdone, possibly because we count as thinking only the most efficient examples of the activity. And perhaps it is only because it seems so natural that the 'reasoning animal' should think that we are impressed by the fact that he does not do it oftener. One has only to recall all the 'useless' thinking that goes on to suspect that human beings think more than is strictly necessary for the preservation of the individual or the race. Intellectual games such as bridge and chess, like some of the more abstract branches of mathematics, science, and philosophy, have no very obvious or immediate utilitarian value to the thinker. And when one observes all the unsuccessful thinking which is persistently and even zestfully pursued, often in rather discouraging circumstances, one suspects that it may be carried on, at least in part, for its own sake.

II. No activity can be considered instinctive unless it is native. That the *capacity* to think is determined by heredity is suggested by the results of intelligence tests, assuming, of course, that intelligence and the ability to think go together. The data on the 'expected' distribution of intelligence in the population, on the constancy of the I.Q., on the character of growth curves, and on the tendency for superior ability and feeble-mindedness to run in families, are usually cited in support of this position.

But the question under discussion is not merely whether thinking is based on a native *capacity*. In a sense, the bare capacity to perform any activity is native. Unless the organism has inherited the necessary structural equipment for an act, be it flying, sneezing, or driving an airplane, the act cannot be performed. The concept of instinct demands that the inherited activity be a more specifically determined mode of response. However, even in this more restricted sense, the evidence from intelligence testing suggests that the ability to think is native. Test situations demand fairly definite modes of response and these responses can be elicited from some organisms and not from others in spite of simi-

larities in training and experience. Furthermore, the impossibility of training some individuals to certain responses which others adopt without instruction and as a matter of course suggests the existence of an inherited basis, present to a greater degree in some organisms than in others, which determines the response more specifically than the mere presence or absence of gross structures.

On the other hand, there is evidence which casts doubt on the all important rôle of native factors. The psychological literature is not without reports of shifting I.Q.'s, and attempts have been made, especially from the sociological point of view, to connect differences in intelligence as tested with differences in environment. For example, positive correlations between Alpha scores and ratings for educational opportunities obtained by different states of the Union [5] have been used as evidence of the importance of the rôle of environmental stimulation in determining the development of intelligence.

It is significant that the intellectual and instinctive functions are in almost exactly the same situation in this respect. Whereas studies of intelligence have tended to show that intelligence is *more* completely determined by heredity and *less* susceptible to environmental influences than was formerly supposed, studies of the instincts have emphasized the point that they are *less* inevitable and *more* modifiable than the older concept suggested. Kuo has taken the extreme position in this respect, regarding even reflexes as learned reactions, and instincts as consequences of the structural equipment which makes some activities more practicable to all members of a species than others.[6] Obviously, thinking can be no less native than instinct if this view of the matter is taken.

It is perhaps unnecessary to say that this discussion makes no attempt to decide the complex question of whether thinking, or any other activity, is native or acquired, or to

[5] Bond, H. M., 'What the Army "Intelligence" Tests Measured,' *Opportunity*, 2, July, 1924, pp. 197–202.

[6] Kuo, Z. Y., 'Giving up Instincts in Psychology,' *J. Phil.*, 1921, 18, pp. 645–664.

separate the native from the acquired factors. It attempts only to show that thinking is not in a different class from the instincts in this respect, that very much the same kind of evidence can be given pro and con on the question. The fact that the two kinds of activities behave so similarly under analysis, resisting it at the same points and presenting similar results at others, suggests that fundamentally they are the same stuff.

III. A third characteristic of instinct, really implied in the preceding discussion, is that it displays sufficient definiteness of organization or regularity of pattern to make it a distinguishable unit of behavior. No claim can be made that thinking is a closely organized instinct; but since, with the possible exception of nursing, no closely organized instincts occur in man, thinking does not differ in this respect from human instincts in general. It may even seem absurd to consider the possibility that thinking shows a pattern of the loosely organized sort. Since genuine thinking involves a reaction to novelty, and since the very point of the process is to meet the specific demands of a new situation, it may seem obvious that no two cases of it can be alike.

But again the recognized human instincts show a similar condition. Probably no two cases of fighting are exactly alike, but involve specific adjustments to the unique features of the particular situation. Yet fighting with fists, with machine guns, and with articulate speech exhibit unmistakable similarities in general outline,—similarities in the situations which give rise to them, in the activities in which they terminate, and in certain characteristics of the activities which intervene between the starting point and the terminus and which make a behavior unit of the whole. The pattern here is limited to general organization and outline, and a pattern of this sort is not hard to find in thinking.

In fact, studies of thinking undertaken from very different points of view have brought out this characteristic. Recently it has been stressed by writers of the *Gestalt* school who have been impressed by the fact that the activity is a continuous whole, having a definite beginning and end and therefore as

much pattern [7] as is involved in that situation. Köhler [8] specifically applies this notion to thinking and distinguishes between a real solution and mere chance success due to random movements in terms of the continuity and unity of the performance. Koffka [9] calls this characteristic 'closure' and regards it as a characteristic of all psychological phenomena, including of course both thinking and instinct. Some of the earlier studies of controlled association [10] have observed a similar condition—an initial set, and a series of relevant activities brought to a close only when the requirements of the task have been fulfilled, but then very definitely concluded. In a recent experiment on human thinking, in which the conditions were very different from those prescribed by Köhler, the present writer found indications of uniform modes of procedure,—fairly regular temporal sequences of reactions involving characteristic modes of beginning a problem, characteristic modes of behavior connected with specific situations during its course, and a very definite and marked reaction which characterized the solution. These procedures were the same from individual to individual, but varied with the kind of problem presented and appeared in the midst of great dissimilarities so far as the specific content of the response was concerned.[11]

Furthermore, something like a general pattern is implied in the very possibility of a normative science of logic, even if it makes no pretense of giving an account of the actual psychological processes involved. The pattern here is not an invariable mode of procedure, but a set of specifications which

[7] 'Pattern' here denotes merely an empirically discoverable arrangement. The theoretical interpretations, which differ in different writers, are not taken into consideration.

[8] Köhler, W., *op. cit.*, pp. 16 and 17.

[9] Koffka, K., 'The Growth of the Mind.' (Translated by R. M. Ogden.) New York, Harcourt, Brace and Co., 1925, pp. 103 *ff.* and pp. 205 *ff.*

[10] Messer, A., 'Experimentelle psychologische Untersuchung über das Denken,' *Arch. f. d. ges. Psychol.*, 1906, 8, pp. 117 *ff.*

May, M. A., 'The Mechanism of Controlled Association,' *Arch. Psychol.*, No. 34, pp. 40–44.

[11] Heidbreder, E. F., 'An Experimental Study of Thinking,' *Arch. Psychol.*, No. 73, pp. 146–153.

any particular procedure must somehow fulfill if it is to be valid. Just as indicative of regular tendencies to think in certain ways is the existence of common fallacies. Indeed, even in the more detailed processes involved in thinking, we find similarities from one individual to another. Experimental literature and everyday observation both show that in some situations we tend to make comparisons, in others to analyze the whole into its parts, and in still others to put two and two together and draw conclusions. These similarities in reaction which occur without specific instructions are just as indicative of natural tendencies to behave in certain ways as are tendencies to flee from some objects and to attack others.

It may be granted that there is less uniformity of procedure in thinking than in most of the instincts, but the difference is only one of degree. We are usually impressed by the novelty of the responses that appear in thinking, but it is just as significant that in the midst of these novelties, there are observable regularities of procedure.

IV. An activity is more likely to qualify as an instinct if it is found in other animals besides man. Current psychological opinion is for the most part in accord with the hypothesis that human thinking is continuous with animal behavior. The many studies which bring out similarities between thought activities and animal trial-and-error may be cited as evidence that thinking does not make a sudden and unheralded appearance in man.

It is of special significance that even when observations are made from very different theoretical standpoints, experimental studies of problem-solving agree that there is no gap between human and animal procedures. The work of Ruger and Köhler, when taken together, give complementary evidence on this point. Whereas Ruger is impressed by the large amount of typically 'animal' trial-and-error which appeared in the thinking of his human subjects,[12] Köhler insists that when animals are not by the very conditions of the experiment thrown back on pure trial-and-error, they

[12] Ruger, H. A., 'The Psychology of Efficiency,' *Arch. Psychol.*, No. 15, pp. 9–14.

show something corresponding to insight and understanding.[13] That is, Köhler found in his apes the very behavior which Ruger considered indicative of insight and which he found to correspond to the sudden drops in time which he regarded as the distinguishing feature of human as compared with animal learning curves.

Many psychologists are reluctant to deal with insight in either human or animal learning, perhaps because it seems to carry extra-naturalistic implications. But both Ruger and Köhler give objective criteria [14] by means of which it may be identified as observable behavior, and it is quite possible to accept it as a distinguishable and as yet unanalyzed response, without any implications as to its ultimate nature. It is worth emphasizing the point that even when thinking is taken in its highest and most 'unreduced' form, there is evidence that it is continuous with animal activities.

V. In man, instincts usually have a pronounced emotional accompaniment, and the regular involvement of affective reactions in a given process strengthens the presumption that it is instinctive.

It is unnecessary to emphasize the presence of emotional factors in thinking. The notion that thinking is a cold, unimpassioned, purely intellectual operation is no longer entertained. Experimental literature is full of evidence to the contrary; witness the testimony from such different sources as the 'imageless thought' papers,[15] with their affectively toned *Bewusstseinslagen*; Ruger's experiment, with its emphasis on the rôle of 'attitudes,' [16] largely emotional; Miss Fisher's study of 'Generalizing Abstraction,' with its introspections reporting "tensions, strains, organic and affective contents"; [17] and Köhler's work on apes, with its descriptions of emotional behavior during problem-solving.[18] The point to be emphasized is that in its emotional side,

[13] Köhler, W., *op. cit.*, pp. 275–279.

[14] Ruger, H. A., *op. cit.*, p. 50. Köhler, W., *op. cit.*, p. 16.

[15] *E.g.* Messer, A., *op. cit.*, pp. 183–186.

[16] Ruger, H. A., *op. cit.*, see especially pp. 36–39.

[17] Fisher, S. C., 'The Process of Generalizing Abstraction,' PSYCHOL. MONOG., 21, No. 90, pp. 40 *ff.* and pp. 191 *ff.*

[18] Köhler, W., *op. cit.*, see especially the behavior of Koko and Sultan, pp. 40–49.

thinking conforms to the pattern of instinct. When its forward movement is checked, it involves pain; when it is progressing successfully, it brings pleasure; when its goal is attained, there is a definite satisfaction.

The pain and difficulty of thinking have been stressed frequently, often with the implication that the process is difficult because it is unnatural and runs counter to instinctive trends. But the conflict between any two instincts is painful and the conflict between thinking and any other trend is only a special case of this general rule. It is possible that as a newcomer among the instincts and as involving a less definitely established mechanism, it may have little chance, unless reinforced, to become dominant over other tendencies. But it is no less instinctive and natural for that. Besides, it is significant that, in thinking, the pain and difficulty appear before the problem is solved and when the solution seems unattainable, just as they do in any thwarted instinct. In other words, emotion accompanies thinking in the regular instinctive way.

VI. Thinking, too, has its perversions. Its drive, again in conformity with the regular instinctive pattern, finds expression in more easily obtained satisfactions, if its natural expression cannot be carried through. One of these perversions, rationalization, has been mentioned in passing and it has been suggested that it involves a concession to the rational as well as to the instinctive demands of the situation, since neither will subside completely in case of conflict. The same view may be taken of day dreaming and other forms of autistic thinking.

There is another perversion, however, which has received less attention as such, but which is probably as significant as any of the others. This is the practice of lying. Like rationalization and day dreaming, lying involves a failure to adjust adequately to objective facts and places a disproportionate emphasis on the subjective demands of the situation; but unlike them, it involves an attempt to operate practically on the environment. In this respect, it is closer to genuine thinking, since it does not stop short of its outlet

in overt action which tries to create a working adjustment between the inner and outer demands of the situation. Furthermore, it often leads to successful adjustments, so far as the social factors of the situation are concerned. Its deficiency as a tool for dealing with the environment lies in the fact that it has a limited use. There are parts of the world, the non-social portions, over which it has no control whatever; and since most lies cannot be maintained in the face of non-social facts which run counter to them, lying is successful only temporarily and partially even in its proper field. The practical objection to lying, therefore, is that it does not reckon with all the factors in the case, a condition which is responsible for the failure of many hypotheses not intended as lies.

Indeed, it is hard to distinguish between lying and thinking except in terms of unanalyzed accompanying processes called 'intention.' As to the actual procedures employed, they are in many respects identical. The invention of a plausible lie and of an effective hypothesis involve the same kind of manipulative activities and the same careful adjustments of facts which seem disconnected or irreconcilable. It is difficul[illegible] to draw a line (except in terms of 'intention') betw[illegible]taining a lie as long as it will hold and in pushing a hyp[illegible]esis as far as it will go, though the latter procedure is recognized as a useful and necessary measure in thinking. In fact, a creature capable of thinking would almost inevitably hit upon the easy and apparently promising procedure of lying in the course of his trial-and-error activities in connection with this loosely organized reaction. The fact that most children do so bears out this conjecture.

The whole question of the relation of lying to thinking would probably repay more careful consideration, but the point to be noted in the present connection is that it stands in the same relation to thinking as perversions do to their respective instincts, and that in so doing indicates that the thinking drive behaves in the regular instinctive way in its susceptibility to irregular forms of expression.

Thinking, then, may be described as a form of native behavior of the loosely organized variety, having a racial history which antedates man, intimate connections with affective and emotional reactions, and a non-rational drive which sometimes finds its outlet in perversions. In other words, it conforms to the concept of instinct.

This statement suggests two obvious objections.

I. The first is that the apparent presence in thinking of the characteristics of instinct may be due to an inability to separate the instinctive and intellectual factors in concrete cases of behavior. In psychological processes in general, it is the exception rather than the rule to find the activity in its pure form. Any given case of thinking, therefore, would include both intellectual and instinctive processes which, if isolated, might be shown to consist of essentially different modes of reaction. In particular, its drive, its liability to perversion, and its conformity in general pattern to the working out of an instinct might be characteristics imposed upon the total behavior unit by the presence of instinctive factors.

There can be little doubt that thinking involves the participation of the so-called instinctive urges. Woodworth [19] recognizes the presence of both self assertion and submissiveness in problem solving; enough self assertion to make one persist in spite of temporary checks, enough submissiveness to make one give up particular attempts in the face of insuperable obstructions. The fighting reaction, too, often appears in thinking. Intellectual controversies give the clearest illustration of this fact; frequently the selection of evidence, the very discovery of evidence, and the relative emphasis given to different sets of data are determined by the fact that a fight is on. Disgust and repulsion also have their effects; the rejection of a hypothesis may be determined largely by a personal distaste for the general point of view it implies. Fear and cautious behavior often appear in thinking. Extreme scepticism has often been characterized as essentially a failure of nerve. In so intimate and essential a part of

[19] Woodworth, R. S., 'Psychology,' New York, Henry Holt and Co., 1921, pp. 287–288.

thinking as the suggestion of hypotheses to be considered, the rôle of non-rational factors is inescapable. Some possibilities are ruled out as soon as they suggest themselves, and sometimes their very appearance is prevented by deeply rooted presuppositions to the contrary, which are probably grounded in herd sanctions and taboos, whether the herd is a social or political organization or a scientific or philosophical school of thought.

Examples might be multiplied, but it must be remembered that from one point of view, this intimate mingling of intellect and instinct is an additional indication that they are essentially alike and that they enter into blends, teams, and compounds, exactly as instincts generally do. The question is not whether instinctive trends participate in and play an important part in determining the course of thought—a fact which is sufficiently obvious—but whether the rational and instinctive factors operate in similar or dissimilar ways and what relation they bear to each other. According to one view, there are two different kinds of activity; the instinctive, which constitutes our drives, and the intellectual, which operates only under the stimulation of our drives and is merely instrumental in bringing about their consummation. According to the other view, both activities would be fundamentally the same kind of response. Both would be drives in the sense of being natural modes of functioning which would tend to get into action in some way and to move toward their own particular satisfactions. Differences in intensity would undoubtedly exist, and there is little doubt that in most cases the traditional instincts are prepotent. But all drives, according to this view, would have some influence on behavior, and not even the weakest could be altogether ignored.

In our present ignorance of the detailed processes involved in both thinking and instinct, it is impossible to prove or disprove either possibility. It is suggested, however, that it is just as plausible, and more suitable to the needs of the present situation, to start not with the idea that there are two distinct kinds of activity, intellectual and instinctive, but with the assumption that the various kinds of behavior that human beings exhibit—fighting, fleeing, seeking food, thinking—are

probably the same kind of activities, sufficiently alike to make it possible to study them according to the same methods, sufficiently different to make it possible to distinguish between the different problems they present. It is significant that the studies of instinct have shown that it is not so instinctive as it was thought to be, whereas the studies of thinking have shown that it is not so intellectual as it appeared. This suggests that we may find the same condition in these activities as has been found in the emotions—no absolutely distinct patterns which can be definitely labeled, but rather general fundamental modes of reaction which may vary through many gradations in concrete behavior. For example, it is often difficult to tell whether a man is primarily afraid or angry; but it is often quite as difficult to tell whether he is primarily thinking or fighting. This suggests that in instinct and in thinking, we may find the same general mechanisms of response, probably involving mainly the striped muscles and the cerebro-spinal nervous system, just as in the various kinds of emotion, we find the same underlying modes of response, in this case involving the smooth muscles and glands and the autonomic nervous system. This would place thinking and instinct in the same general class, and such activities as thinking, fighting, and fleeing would differ from each other to the extent, but only to the extent, that fear, rage, and love do.

II. The second objection is that the whole concept of instinct is outworn; that it has proved misleading even in the field where it seemed most clearly to apply. But the objections to the concept of instinct as applied to the instincts do not hold of its application to thinking. The objections there were mainly two: first, that it gave an erroneous impression of the facts by overemphasizing the uniformity, unmodifiability, and ready-made character of the response; and second, that in doing so, it created an attitude which took the activities for granted and therefore failed to encourage investigation. But the use of the same concept in the field of thinking would have the opposite effect. By emphasizing the biological nature of the process, it would mark the material as suitable for scientific treatment and

would help to do away with a certain reluctance which now exists in some quarters to deal with this particular psychological process. And by suggesting that the activity has 'pattern' and 'drive' in the same sense that other complex units of behavior do, it would call attention to facts about thinking which are in almost as much danger of being overlooked as the lack of uniformity and perfection in instinct. Its adoption would be useful as a temporary measure, which is the only justification any working hypothesis can or need claim.

Indeed, the whole point of regarding thinking as an instinct may be stated in terms of its practical consequences.

From the point of view of experimental investigation, it would make possible a more straightforward program of research. The problem of thinking could be met as simply and directly as any other psychological problem. Thinking could be approached from the naïve and empirical and scientific standpoint that such behavior occurs—that it can be observed, and that it may be described, analyzed and explained in the same way and by the same right as any other complex organic process.

From the point of view of understanding and controlling human behavior, it would help to clear the confusion which now exists concerning the part played by intellectual factors in determining conduct. It would recognize the serious mistake involved in the old notion of the rational man, but it would discover a similar mistake in going to the other extreme and refusing to consider the possibility that the capacity to think may make some demands on its own account, which, along with other drives, have an effect on an individual's reactions. By emphasizing the fact that thinking is quite as 'natural' a process as any other activity which the organism is equipped to perform, it suggests that in so far as we are trying to huddle intellect out of sight, we are making the same kind of mistake our ancestors did in trying to leave sex out of their calculations. It implies that we can understand human nature not by reducing it to a single all-powerful urge, but by recognizing a plurality of drives of varying degrees of intensity, no one of which has gained complete ascendency over the others.

IS A SYNTHESIS OF PSYCHOLOGICAL SCHOOLS TO BE FOUND IN A PERSONALISTIC ACT-PSYCHOLOGY?

BY HORACE B. ENGLISH

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I. While it is doubtless idle to hope for complete harmony among various schools of psychological thought, we cannot safely abandon the effort to discover a formula which will include the *positive* implications of all or a large number of such schools. The ultimate justification, of course, of a systematic viewpoint is its power to arrange and coördinate a vast body of facts. But before undertaking so monumental a task, it is not an idle exercise in dialectic to consider how well the psychology of act agrees, especially at critical points with other systems. For if we can thus include the essentials of rival systems, we shall have found a viewpoint which will take care of the facts upon which they lay most stress.

Since cognition, and especially perceiving, is the test of a psychological system, we shall take that as our point of departure. Immediately, of course, we face the dire specter of epistemology. Let us be frank about it. Psychology, especially the psychology of cognition, is inevitably and intimately related to epistemology just as physics is related to cosmology. Now epistemology may not be allowed to determine psychological system. But it is fair to ask of psychology that it state its facts and its laws in such a way that it does not seem impossible that we know anything at all. A psychological system which raises unnecessary epistemological difficulties is to be provisionally condemned.

It is easy to say that we must let facts, and facts alone, control; but it is no longer easy to say this with a sober face. We are by now sophisticated enough to realize that the criteria of 'fact' are far from simple and that all facts are by nature

colored by interpretation. Facts, to paraphrase an expression of Ritchie's, are merely philosophical interpretations of a few generations back, mingled with a few even mouldier superstitions to give that homely look so dear to the every-day mind.

On the other hand, a reasonable peek into philosophy will do psychology no harm; indeed it would seem to be almost an essential to its remaining psychology. With this polite bow to the ferocious bogy of being merely philosophical, we may turn to what we mean by a psychology of act.

II. The task of psychology we take to be the description and explanation of personal experience and of the results of experience. By experience we mean a personal act, a relating of self to object, a sort of enjoyment as S. Alexander and Lloyd Morgan call it, *ein Erleben* rather than *ein Erlebtes*. Process, were not the term so divergently used by the American psychologists of 'content,' would be an acceptable synonym. All of these are so many attempts at an indication of what is *sui generis*, apparently, and hence incapable of exact definition. The act, as the new realists once said, is a unique relation between a subject and an object, taking the latter term in its widest connotation and the former at the lowest level of philosophical sophistication. If we consider briefly the contrast between the act and its two determinants, its nature may be clearer.

The object first. And to begin with, when we designate a tree, a virtue, a concept, or a social situation as an object, we do not imply that it is either mental or non-mental, not being quite sure what these terms mean in this connection. (Or rather being quite sure that they mean different things to different people.) The relative independence of the object from the individual knower, which is certainly a datum, is sufficient to provide the contrast here made. The object's specific character for psychology is indicated in the tautological expression—which is certainly not merely tautological —objective. Objects, we shall therefore say, are objective, while the act at least *as such* is not. It is entirely open to philosophers to contend that ultimately the act constitutes or

creates the object; or, on the other hand, that the object is independently real. The essential point for psychology is the way the object, especially in perceiving, stands over against and partially determines the act.

While we may thus put to one side either as deeply metaphysical or as superficially terminological the question as to the 'mental' character of the object, we cannot so easily dispose of the question of what to do with it. Many of the act-psychologists have thrown it overboard. S. Alexander, for example, in accord with his realistic position, asserts that the object is non-mental and is studied in reference to the psychic act with the same relevance as the nervous system or physiological chemistry; it is merely adjuvant.

Such a position is open to some criticism. In the first place it excludes much that has historically occupied the science and all that some very active psychologists call psychology. To say the least, such exclusion is a tactical blunder. Moreover, no other science than psychology can furnish knowledge about the objects of the psychic act unless we are to create a special discipline of *Gegenstandstheorie* for that purpose. But it yet remains to be demonstrated that *Gegenstandstheorie* can obtain any important body of facts by other than psychological methods, or that its findings have other important bearing than in a psychological setting. It seems to be, in short, essentially a branch of psychology, somewhat contaminated by logic. Further, such facts as the *Gestalt* group are furnishing definitely show the danger of separating the study of the act from the study of the object. And indeed it takes but slight consideration to show that the psychic act is essentially meaningless unless one includes the object. The verbs which best designate the act are transitive verbs. To say, 'I perceive' or 'I think' is incomplete; I perceive colors, sounds, rhythms; and, whatever their analytic nature as imaginal or imageless, I think thoughts. Psychology must consider not only the objective reference of the act; it must also consider the object of reference.

Accordingly, the psychology of act must include as an integral part of its subject-matter a study of objects as

objects. If we are to remain on the empirical level, however, we must reject the view, concurred in unfortunately by several of the act-psychologists, that the 'object' is a sort of mental content which sustains some sort of relation—to be determined by philosophy—to the real object. The object concerned in the psychic act is, for psychology, the real object, though taken from a personally limited viewpoint. The *perceptum* is the *percipiendum*, or part of it. (The *percipiendum* must not, however, be identified with the so-called 'physical object,' which has a much narrower meaning.) How this is possible and how it is to be interpreted is the task of metaphysics, of course. Ours is the job of describing the varying degrees of completeness and accuracy with which we cognize the object.

This may fairly be called psychological realism, but it is a realism which does not prejudge the philosophical question. And it is a psychology hospitable to all the experimental findings of those who define psychology as the study of 'mental content' or of the 'world of objects with the man left in.' The psychology of act when properly conceived is at the same time a psychology of content, or at least includes all its data.[1]

III. We have said that the act stands over against the object and is partly determined by it. But it likewise stands over against and is partly determined by the subject or self. We use the term 'self' in this connection because it is on the whole emptier of specific connotation than any of its various quasi-synonyms. What the self of the psychologist is, is an empirical problem and the central one for psychology to solve.[2] But although it is empirical, it is not mainly a phenomenological problem. Miss Calkins, to be sure, contends that we are directly aware of the self in all experience. In company with St. Augustine and the goodly fellowship of the Scholastics, she insists on the *con-* in consciousness.

[1] Dunlap's 'System of Psychology' may well be adduced as evidence on this point. Although his definition runs in terms of act-psychology, his emphasis lies heavily on the side of 'content.' See particularly his doctrine of thought and of feeling.

[2] The adjective personal as used in our statement of the task of psychology refers to the self. The term person seems to the writer admirably to bring to expression the real nature of the subject or self; but this is to anticipate empirical study.

It is not necessary to deny that we do often have something which is genuinely an observation of the self. Miss Calkins has adduced much empirical evidence for this and an even greater mass of indirect evidence. But we learn a great deal more about the self-being-conscious by inference than ever we do by direct observation, and what we thus learn is more likely to be free from bias and rationalization. Psychopathology, with its profound disturbances of personality or selfhood seems to be particularly instructive. But every mental law, indeed every mental phenomenon affords us opportunity to infer the nature of the self which is involved in such activity.

The writer is painfully aware of a certain circularity in the argument at this point. We began by using the term 'personal' to show what we mean by the act which is the subject-matter of psychology. Now we are engaged in defining the personal by reference to the acts which proceed from the self. The procedure can be justified logically only if one or both terms possess a certain degree of self-evidence. That is, in fact, the case. Yet we ought not to dodge the issue; although we do know vaguely what is denoted when we speak of the subject of experience, the self, or the person, we do not by any means know adequately the nature of this self. If no attempt is made here to supply that lack, it is because the problem is, as was indicated above, logically central and practically the most important problem set for psychology; by the same token, it is one not lightly to be undertaken. Our purpose here is the more modest one of pointing out that a psychology which is based upon the act necessarily takes into account and includes the empirical findings of self-psychology.

IV. What McDougall and Nunn have christened hormic psychology is, in all its forms, naturally expressed in terms of act. The intention which Binet found at the bottom of thinking, McDougall's purpose, Thurstone's desire, the wish of the psychopathologist, the conation of Alexander, all of these are essentially acts. But none of them quite serves to mark off the psychic. One and all, they are too crisp and

definite in meaning; and though anti-intellectual in intention and application, they are too intellectualistic in definition and hence too limited in scope. They name certain kinds of act only. And no matter how emphatically their users disclaim taking sides, such terms bias our thinking, whenever our attention wavers, in favor of teleology. There is something, however, in even the most 'passive' sensory receptivity which relates it to conscious purpose and to everything between these two extremes. That something it seems to us is the fact that all are personal acts, expressions not so much of a purpose (which is more often than not absent and may be a mere rationalization when present) as of a person.

The worst crime in these days is to be naïvely optimistic. Yet it does seem as if such a formulation, though acceptable to the defenders of hormic psychology, should not be offensive (unless indeed for that reason!) to those who protect the holy ark of strict and scientific determinism. The facts do not obviously compel us to the view that we are moved exclusively by a *vis a tergo* or by a *vis a termino*. Sometimes the initiative in every important sense seems to come from within the person or self (or organism if that term seems more tough-minded and scientific), sometimes from the environment. But always if we dig down, we find a person acting in relation to his circumstances. That seems to us the fundamental fact from which the interpretations of teleology and mechanism take their common origin.

V. No great difficulty is found for an act-psychology in accommodating functionalism; for although the name has rather gone out, functional psychology lives on under the guise of 'moderate behaviorism' and 'organismic psychology.' Functionalism began as a revolt against the exclusively introspective study of 'content.' As regards introspection, it was brought to its ultimate, if not logical, conclusion in behaviorism. In this development, however, the other implications of functionalism were not only neglected but directly opposed. For behaviorism, as projected by Watson, is as much a study of 'mental content' as is Titchenerian structuralism; the difference lies in the method and in the definition of the mental.

But at its inception, functionalism was as much, or more, a protest against the relatively sterile study of content as it was against the introspective study of that content. The ambiguity which lurks in the term consciousness, however, largely concealed the fact that structuralist and functionalist did not give to consciousness so much a different connotation as a different denotation. By consciousness the structuralist meant and still means primarily what we have described as the object, while the functionalist meant—usually—the act of experiencing such an object. But the functionalists were too much the children of Anglo-German associationism to break completely free and the distinction of act and content in their writings remained largely implicit to the detriment of clear thinking. The entire issue was further confused by the notion of value which is also embedded in that thoroughly equivocal word function. But this notion, also, when thought through is found to be appropriate, not to the objective content, but to the act.

VI. To attempt to enlist the behaviorist under the banner of an act-psychology may seem like inviting a bull to take tea in a very fragile china shop. Yet clearly that is where the behaviorist belongs. In rejecting introspection, he has rejected the traditional criterion of the psychic so that there have not been wanting those who have wondered on what basis he considers himself a psychologist. But for the psychologist of act, whether the psychic is ever, or always, or never, introspectible is not a fundamental point. What makes a thing psychic is that it is a personal act.

Now this is just what the behaviorist has in mind in his distinction of behavior from physiological processes. Behavior he defines as a 'response of the complete and integrated organism.' Now of course the phrase taken literally is nonsense. No act is ever a response of the 'whole animal'; no organism is ever wholly integrated. Integration is a matter of degree. Even if we are to admit that the distinction between physiology and psychology is merely one of degree, what are we to think of a criterion which would assign to psychology the digestion of a good meal (certainly a reason-

ably integrated response of the organism) and to physiology all the phenomena of mental dissociation (in which the integration is certainly relatively low)? No, muscle twitches are to be included in psychology, not when they are responses of the whole animal (which they never really are), but when they form part of a personal act. The further ascription to such acts of a purely materialistic and mechanistic nature belongs to behavior-philosophy, not to behavior-psychology. To act-psychology as to any other, this is an irrelevance.

VII. There remains to be dealt with the newest of new psychologies, *Gestalt*. None of this school interprets its interesting experiments in terms of act and content.[3] Yet that interpretation lies open at hand and makes intelligible its contentions in a way no other does.

To say, as they do, that a configuration is more than the sum of its elements is self-contradictory so long as we consider the grouping from what may be called an internal viewpoint. But in relation to an act in some sense external to the pattern, the latter may be very much more than an aggregate. Water has qualities such as wetness, ability to quench thirst, and so on, only in relation to something else; in itself it is merely H_2O. We may take it as established by the findings of this group of experimenters that the act of perceiving (or any other act) is related to, or intends, not mere aggregates of objects or their elements, but the total pattern of objects which is contemplated or experienced. There is thus no one-to-one relationship between physical stimuli and the acts of perceiving, nor between hypothetical 'subjective' sensation elements and the acts of perceiving. Perceiving is related to the total configuration.

Sensations, according to this view, are definitely objective. They are patterns of great simplicity reached by taking an analytic (and generally artificial) attitude. The pattern of sensation is merely one dominated by quality and intensity,

[3] The Graz school of course does interpret the *Gestalt* in terms of act and content. But it explains the configuration as produced by the act and imposed on the mental content; an explanation foreign to the central *Gestalt* school, which insists on what we may call the objective character of the configuration.

as the pattern of movement is dominated by extent and succession. We can no longer regard sensations as the elements of which percepts (or as we should prefer to say, perceptual objects)are made. Sensations are the simplest configurations, that is, the simplest objects we perceive. The more elaborate patterns are usually not made up of sensations but of the same kind of stuff, if we may so speak of it, as constitute sensations. The ultimate elements constituting the configurations are the so-called attributes,[4] though perhaps there are other attributes to put alongside the traditional 'attributes of sensation.'

But neither Miss Calkins nor the *Gestalt* people have committed themselves to what these are the attributes of; and no desire to avoid philosophical entanglement can justify our leaving our ultimate elements (of 'content,' be it understood) thus hanging in midair. Quite simply, these are the attributes of objects, the elements out of which objects are made by varying combinations and configurations. If anyone wishes to think of them as Platonic ideas, as ultimately real, as emanations from an Absolute or anything of the sort, it does not appear how he is to be prevented; and as Knight Dunlap says in a similar connection, such assumptions can be prevented, with a little care, from doing any injury to his psychology. But psychology must remain rather nominalistic. We must discover the attributes which objects disclose, and then proceed to study how the total object or configuration alters when various changes are made among these attributes. Such is one of the major tasks of psychology and the *Gestalt* group have brought us measurably nearer to its solution.

VIII. We may now call the roll of those whose positive contentions we have sought to comprehend under a personalistic act-psychology. Here, surely, is Heaven's plenty. The psychologist of 'mental content,' inheritor of Anglo-German associationism, and the conational psychology of S. Alexander; hormic or purposive psychology, and behaviorism; the functional psychology of the turn of the century, and the

[4] As Miss Calkins has for many years insisted.

Gestalt of today; the self psychology of Calkins, Lipps, and Stern, and the less systematized implications of most psychopathologists. What has been left out? Their negations, their denials, the things they do not see or will not have! But not a single experimental finding, not a single empirical fact, of all these schools is in principle excluded from psychology thus conceived. To prove that each such fact has orderly place in such a psychology is the task of no mere prolegomena but of a full quarto or folio system of psychology.

WHAT IS APPLIED PSYCHOLOGY?[1]

BY MAX FREYD

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The use of the term 'applied psychology' by authorities in pseudo-science, such as mental healers or leaders of cults, is a constant source of annoyance to psychologists. The condition is aggravated when people who are intelligent and in other respects well informed ask the trained psychologist to judge their character, tell them how they can concentrate, read their palms, or interpret their dreams. Misunderstandings like these are common among college students when entering courses in psychology. Thanks to the publicity given to the term by the numerous advertising lecturers, there is no longer any doubt that more people define 'applied psychology' as a cult than as a science.

There are times when our indignation at these people who pirate the term must be tempered by a feeling that perhaps we ourselves are to blame. Are we agreed among ourselves on a definition of applied psychology? Engineering and medicine are applied sciences and yet there is no popular misuse of these words. The average laymen knows pretty well what doctors and engineers are and what they do. Perhaps if the scientific meaning of applied psychology were as well established, there would not be so much misuse of it among laymen.

It is true that there are more reasons for a popular misunderstanding of applied psychology than of other applied sciences. Psychology itself is a young science, its established content is not extensive, and it has had internal dissentions. Its subject matter has an intense human interest. People want a short cut to health, happiness and success. Scientific investigation has not been able to gather information fast

[1] Read at the Ithaca meeting of the American Psychological Association, December 29, 1925.

enough to satisfy public demand. Borderline investigators such as Mrs. Eddy, Coué and a host of others have stepped into the breach and have excelled psychologists of standing as missionaries for their ideas. No wonder the basic concepts of this science are hazy to the layman!

Is there a concept to which the term applied psychology belongs? If so, is this the proper term to employ? Perhaps we have given a name to something which we have imperfectly defined, allowing the pseudo-scientist an opportunity to make his own definition. In that case, it were best to drop both name and concept, and develop if possible the usefulness of psychology under a different name.

At some future time the justification for a separate applied science may be stronger. At present there are good reasons for discarding applied psychology both in name and concept, and including under the wing of such terms as general, educational and industrial psychology, everything which it has legitimately been made to cover. The fact that it has come to have an unscientific popular meaning lends weight to these arguments for its discard.

Writers and authorities on the practical applications of psychology have drawn distinctions between general psychology and applied psychology with regard to method and technique, content, and viewpoint. Let us take up these distinctions one at a time and consider their validity.

1. *Method and Technique of Measurement.*—It is claimed that applied psychology is the application of psychological method to problems of everyday life.

Psychological experimental method is but an illustration of scientific method in general. This method usually starts with an hypothesis, devises methods of measuring the variables under consideration, makes these measurements under controlled conditions, interprets its data, and draws conclusions. Sciences differ in regard to their subject matter, but not their adherence to these standards of method. To quote from Pearson's 'Grammar of Science,' "The scientific method is one and the same in all branches, and that method is the method of all logically trained minds. . . . The unity of all science consists alone in its method, not in its material."

Although sciences employ the same method in general outline, they may differ in their technique of measurement. It is hard to see how any experiment in applied psychology differs in this respect from an experiment in general psychology. Vocational tests are very much like the ordinary apparatus in use in laboratories. Rating scales have their use in the laboratory as well as in industry. A cursory glance through a text on applied psychology reveals no essential differences in materials of experiment from those in a text in general psychology.

General and applied psychology are apparently not to be distinguished in regard to method.

2. *Content.*—Is applied psychology the application of the content of general psychology to the affairs of daily life? Does it differ in content from general psychology? There are reasons for giving a negative answer to both of these questions.

The content of general psychology is established by laboratory experiment, in which one variable at a time is isolated and its relationship to other variables is established. The carefully controlled conditions which are essential to an experiment of this sort are rarely met with in daily life. Almost all human behavior is aroused by a complex of stimuli. How then can the psychologist use this information in everyday life? Even if he knows the exact effect of one stimulus how can he judge the effect it will have in a situation involving innumerable stimuli? Even if he knows the isolated effect of each stimulus in a complex situation, how can he judge their relative strength and mass effect? How does he know just when a certain fact or principle of general psychology is applicable? The application of psychological content to daily life may be possible, but it borders too much on the subjective to have a very sound scientific standing.

The emphasis placed by popular books on the applicability of findings of psychology to practical situations has given the layman a false sense of security in the use of these findings. Experiment shows that red and yellow are the most popular colors among adults. But one cannot apply such content.

Will men invariably buy red ties or yellow suits? All one can say is that when pieces of paper of identical size and shape but different color are presented to groups of men or women certain colors will be chosen more often than others. Material, cut and style have been left out of the study. Laboratory experiments on memory and attention value cannot be applied to advertising since control is not applied to such variables as use of headline, coupon, or illustration, demand or need for the product, reading or advertising material adjacent to this advertisement, variations in appeal, or cost to the advertiser of space and production.

The content of general psychology then cannot be *applied* to daily life but merely indicates a road to follow in further investigations. Every fact of psychology to be used in a practical situation must either have been established for that situation or validated for it. This means that the same or similar experiments or follow up should take place in daily life as in the laboratory, using the same method and the same tools.

This leads to the second question: Does applied psychology *differ* in content or subject matter from general psychology?

Applied psychology is commonly thought to deal with the practical affairs of daily life, such as the education of children, vocational adjustment, advertising, mental health, efficiency in working conditions and the like, while general psychology deals with more abstract problems.

If the working day is to be set off as a field for applied psychology, the eight hours of rest as a field for the psychology of relaxation, and the eight hours of sleep devoted to psychoanalysis, nothing remains for the general psychologist. All human behavior, whether it be in the home, school, workshop, or playing field, is subject matter for general psychology. Such divisions of subject matter as industrial, educational, or abnormal psychology come under the head of general psychology and not applied psychology.

3. *Emphasis.*—It is asserted that the purpose of science is description and explanation, of technology, prediction and

control. If applied psychology is a technology then we shall have to consider the validity of this distinction.

Control may mean two things. It may mean (1) power which is in the hands of representatives of organized society, such as the judiciary, the police, the army, and governing officials, or in the hands of business executives. Such power of control is rarely vested in trained psychologists. Another form of control (2) is advice furnished to those with power over human beings, where this advice is commonly acted upon. The psychologist is more likely to be in possession of this form of control.

Does the possession of either of these forms of control warrant the title 'applied psychologist'? Success in control of the first type is due to technical information made available to the executive, or to a special ability or type of personality which facilitates control. Neither is science in the strictest sense. Control of the second type is not reliable unless the information is discovered experimentally or is the validation of previous psychological discoveries. If these steps are carried out the 'controller' is entitled to be known as a psychologist and not as an applied psychologist.

Another distinction in viewpoint often drawn between general and applied psychology is that the applied psychologist is constantly under the influence of economic motives, attacking problems the solution of which should bring about practical economies through the conservation of human energy. While there may be a distinction in this respect it is a quantitative and not a qualitative one. No one who is in the employ of others is entirely free from the economic motive. It can be seen in back of even the 'purest' experiment in psychology. The psychologist in industry is of course more strongly influenced by the desire to reveal facts which are economically sound and useful than is the laboratory psychologist, but this is a matter only of degree.

Although psychologists vary in the degree to which they are stimulated by the desire to obtain economically valuable results, they always give the same response, the response which has become well established through their training and

which stamps them as psychologists and scientists—the application of the scientific experimental method to the problem in hand. This method is the same for a pure and an applied science. There is a difference of degree in the stimulus to psychologists working in different fields, but the response is the same.

It makes little difference how strongly the economic motive affects the choice of problems, for, after all, every result of experimentation has some economic value. Often this value cannot be foreseen when the experiment is planned. A laboratory experiment initiated with slight reference to economies may yield more practical results than an experiment conducted in industry. There is no necessary correlation between this economic value and the desires of the investigator or the type of problem he attacks.

Yet another distinction in viewpoint is implied in the following quotation from Hollingworth and Poffenberger's 'Applied Psychology,' "The fundamental fact of applied psychology is that the individual is the unit of action, and all advance in this science must rest upon a knowledge of individual behavior." As it stands, this statement hardly differentiates applied psychology from general psychology, for it applies equally well to both. It is unsafe to assert that applied psychology emphasizes individual differences rather than general laws. Studies in ventilation are certainly not prompted by an interest in individual differences. Neither are studies in vocational selection. Their aim is to establish the relation of vocational effort to atmospheric conditions or the probability of vocational accomplishment implied by certain test scores. Any individual's behavior under given circumstances can be understood and predicted only when it has been established that human beings behave in a characteristic manner under these circumstances. When general relationships have been discovered between stimulus and response, the behavior of the individual may be predicted from the stimulus. These relationships, whether limited or general in significance, have to be established for an industrial problem just as for a laboratory one.

The scientific distinctions often drawn between applied and general psychology are seen to break down under analysis. Since everything ordinarily included by psychologists in the term applied psychology is shown to belong under general psychology, applied psychology has no real scientific connotation and hence may be turned over to pseudo-scientists with little regret.

Although psychology cannot be applied, it can and should be made a broader and more practical science by enlarging its scope and making its experiments where people work and play as well as in the laboratory. The psychologist should be thought of as one who studies human behavior under all conditions without exception. He busies himself in the college psychological laboratory, in commerce and industry, in the school, and in the psychological clinic. When his experimentation requires expertness in other sciences as well as psychology, he is sometimes given such names as physician, psychiatrist, educator, or personnel director. More satisfactory terms will have to be invented to take the place of these, since they do not bear the experimental connotation. The psychologist is differentiated from all others who use psychology in that he constantly maintains the research attitude, putting every fact to the experimental test, and recommending no application of findings which are not verified for the problem in question.

A PRELIMINARY PERSONNEL STUDY OF PSYCHOLOGISTS

BY HARRY DEXTER KITSON

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In introducing vocational guidance into the field of psychologists (an undertaking which has been considered by the National Research Council to be serious enough to warrant publication of a special bulletin devoted to the subject),[1] one of the first questions encountered is 'How great is the turnover among psychologists'?

When a Department of Personnel begins operations in an industrial establishment it assumes as its first task that of determining the rate at which the members of the working force leave the firm and have to be replaced by new workers. The method of determining this consists in using as divisor the average number employed over a given period—week, month, year—and as dividend the number of employees who left the establishment. The quotient, in per cent terms, is called the turnover index. For example, if an average of 1,000 persons are employed throughout the year and 500 resignations are recorded, to be filled by 500 other workers, the turnover index is 500 divided by 1,000 or 50 per cent. (In actual practise the matter is not quite so simple as this would indicate. It may be that some of the vacated positions are not filled, because of retrenchments of one sort or another. Again, some changes are made within the organization itself which require adjustment of personnel but which do not rob the firm of any workers. Some workers may go on leave of absence; some may be on prolonged sick leave. All these circumstances may require modification of the simple formula given above.)

[1] 'Psychology as a Life Work,' National Research Council, Washington, D. C. 1923.

As was said above, one of the most important questions relating to the personnel of psychologists is 'At what rate do they move about in the field of psychological employment'? The membership of the American Psychological Association is now as large as that of an ordinary-sized factory, and so it seems that statistics gathered with the techniques employed in industrial establishments would have significance toward the gradual accumulation of information which will ultimately enable us to administer vocational guidance among psychologists.

With this end in view the members of the Association were considered as employees of a single group among whom it was desired to study the migrations from one institution to another and the number of promotions granted each year to those who remained in one institution. The period from 1920 to 1925 was chosen for study. It was thought imprudent to consider any years previous to 1920, since for at least three of those years there was great unsettlement in the profession due to the war-time activities of psychologists.

The data for the investigation were taken from the yearbooks of the Association for the above-mentioned years. For example, the record of each person as it appears in the 1920 yearbook was compared with his status as recorded in the yearbook for 1921. Count was made of the number who had changed positions from one institution to another and the number who had been promoted in rank within the same institution. In order to secure an index of turnover, the number of persons who had changed institutions was divided by the number of names in the 1920 yearbook (after deducting those who had died or resigned from the Association and whose records therefore did not appear in the book of the next year). The index for promotions in rank was secured in a similar manner, that is, by dividing the number who had received promotion by the net number in the book of the previous year. This procedure gave results as shown in Table I and Fig. 1.

Here it is seen that from a point of ten per cent in 1920 the index based on resignations decreased until in 1923 it

TABLE I

PERSONS LISTED IN YEARBOOKS OF AMERICAN PSYCHOLOGICAL ASSOCIATION, WITH NUMBER WHO RESIGNED AND NUMBER WHO RECEIVED PROMOTION

Yearbook	Number Listed	(Subtract)	Number who Resigned	Percent Resigned	Number Promoted	Percent Promoted	Number Women	Percent Women
1920	393	6	40	10	27	7	61	16
1921	424	4	38	9	18	4	69	16
1922	442	5	28	6	29	6	70	16
1923	457	16	23	5	22	5	79	17
1924	464	6	40	9	17	4	75	16
1925	471						84	18
Average per year...				8		5		

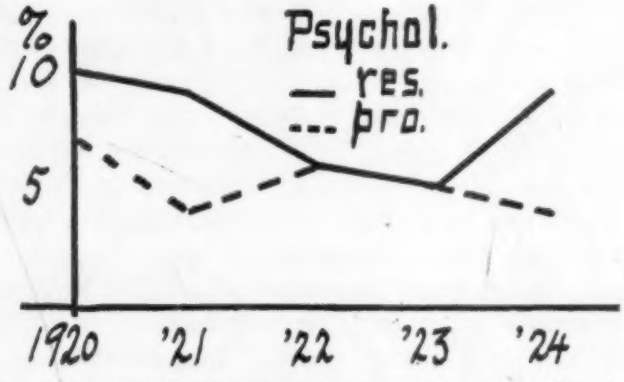

FIG. 1. Per cent of resignations and promotions among psychologists

reached five per cent, but in 1924 it rose again to nine per cent. The index figures based on promotions do not show such wide fluctuations over the five-year period, though a slight downward tendency is noticeable. The apparent unsettlement during the first two years of the period are probably attributable to the changes wrought by the war. Salaries of college teachers, who constitute 80 per cent of these psychologists, did not increase simultaneously in all institutions. Those institutions in which they rose first were successful in inducing certain persons to resign their positions. At the same time this furnished pressure which caused the more tardy institutions to make promotions in rank, which of

course usually meant increase in salary. It is interesting to note that with the exception of two years (1922 and 1923) the number of promotions is always smaller than the number of resignations. For these two years the proportion is the same. This offers interesting ground for speculation. As is well understood by workers, employing establishments make promotions only with great reluctance. This is denoted in these figures where the promotions lag behind the resignations. In a given institution it is necessary for a number of workers to resign before the rest are promoted. What is the number of promotions that an institution must grant in order to keep the resignations of psychologists down to a minimum? Under the conditions of this investigation, the number appears to be five or six per cent. It will be noted that in 1924 when the number of promotions fell below this point, the number of resignations rose.[2]

It would be interesting to compare the turnover among psychologists with that found in industrial establishments. The latter is in general much the higher. Figures given the writer by one of the best manufacturing plants in the metal trades industry, show a turnover for 1924 amounting to 40 per cent. Figures drawn from the industrial world, however, are not strictly comparable with these figures relating to psychologists, since among unskilled workers the turnover is usually higher than it is in a vocation requiring greater skill and longer training.

A fairer basis of comparison would come from the rate of turnover in a single establishment for higher education, since 80 per cent of the members of the American Psychological Association are employed in such establishments. This the writer has computed for X—College, using the college catalogue as he used the yearbook of the American Psychological Association. The yearly indices of turnover are shown in

[2] A study made in six small middle-western colleges showed an average rate of promotion within the same institutions during the years 1921–1924 amounting to two per cent per year. See Brooks, W. S., 'The College Teacher—His Expectation of Continuance and Promotion on Certain Faculties,' *Education*, June, 1925, **45**, pp. 577–585.

Fig. 3, where they range from eight to seventeen during the five-year period. The curve does not resemble very closely that based on the turnover among psychologists. It is to be noted, however, that the curve for promotions within the institution follows at some distance below that for the resignations. As should be expected, the greater number of resignations within this institution occur in the sub-professorial ranks. Among the teachers of professorial rank the turnover ranges between two and three per cent during the five years.

Perhaps the best figure with which to compare the turnover among psychologists would be secured through a study of the members of a similar scientific body. Such a group was sought in the American Society of Zoölogists, which appears to be comparable to the American Psychological Association, working in the biological field, with a high requirement of research for membership, and in size only a little smaller than our Association, the membership in 1925 being 367 as compared with 471 in the American Psychological Association. The official lists[3] of active members were examined in the same way as the lists of the American Psychological Association were examined, with results as shown in Table II and Fig. 2.

TABLE II

PERSONS LISTED IN MEMBERSHIP OF AMERICAN SOCIETY OF ZOÖLOGISTS, WITH NUMBER WHO RESIGNED AND NUMBER WHO RECEIVED PROMOTION

Year	No. Listed	Subtract	No. Who Resigned	Percent Resigned	No. Promoted	Percent Promoted	No. Women	Percent Women
1920	314	9	15	5	22	7	29	9
1921	327	7	9	3	22	7	27	8
1922	357	3	12	3	17	5	31	9
1923	377	5	26	7	17	5	33	9
1924	375	20	17	5	20	6	31	8
1925	367						30	8
Average per year...				5		6		

[3] *Anat. Rec.*, Wistar Institute of Anatomy and Biology, Philadelphia, 17, 20, 23, 24, 26 and 29.

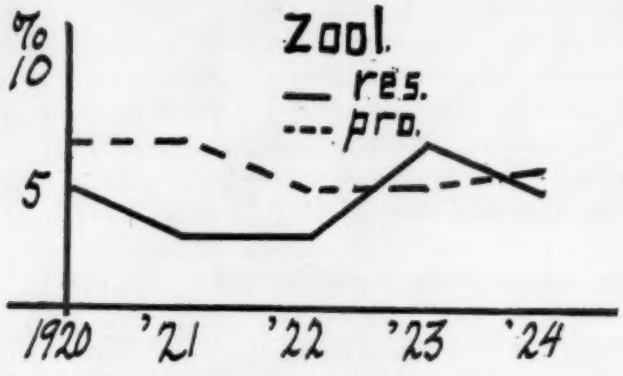

FIG. 2. Per cent of resignations and promotions among zoölogists

It will be noted that the shape of the curve of resignations is, with the exception of the rise in 1924, similar to that for the psychologists. The per cent, however, is smaller. The curve for promotions, is, like that for promotions among psychologists, very close to a straight line. The two outstanding differences between these two groups of scientists are (1) among the psychologists—the proportion of resignations is greater than the proportion of promotions, while among the zoölogists the proportion of resignations is smaller than the proportion of promotions; (2) among the zoölogists—the resignations are noticeably fewer than those among the psychologists. This may indicate that the number of openings for zoölogists is smaller than that for psychologists; that there are not many places where a zoölogist can go if he leaves his present position. On the average the turnover figures for the five-year period are: Psychologists, eight per cent; zoölogists, five per cent. It is interesting to note in this connection that the turnover for about two thousand teachers in schools of engineering during the period 1920–1924 averaged ten per cent per year.[4]

Among other facts which one would like to have before deciding on the vocation of psychologist is a statement showing the absorptive power of the vocation. Can we state

[4] Preliminary Report, Society for the Promotion of Engineering Education, N. Y., June, 1925, p. 17.

in quantitative terms how much of a future there is in the profession? Naturally we shall find no single index to this. The answer will have to come in several kinds of terms. Some light might be secured from an examination of the curve of growth of the Association during the 43 years of its existence. The Secretary was unable, however, to furnish such records. From information given in a yearbook of the association kindly furnished by C. H. Stoelting and Company, the growth of the past ten years can be computed. In the 1915 yearbook, 295 names were given, in 1925, 471, a growth of 176 or sixty per cent in the decade. Of course, we must admit that the American Psychological Association does not contain all of the psychologists of the United States. (It is estimated by Mr. Stoelting, that there are probably 1,200 or 1,500 persons in the country making their living by psychological work.)

A woman would like to know what are her chances in the field. This will be partially revealed by computing the number of women on the list from year to year. Here, again, there is a slight source of error in that a few women are listed as members of the Association who have been married and so have ceased to work in the field. Their number was reduced, however, in the 1924 yearbook (after the dues were raised) and so the present figure, 18 per cent, is at least a minimal index of woman's part in the field. The number of women in the American Society of Zoölogists constitutes (1925) eight per cent of the whole, and has remained at approximately that figure during the five years covered by this investigation (Table II).

Further light on the relative position of women comes from a study of the number of women mentioned in American Men of Science. Dr. Cattell has kindly furnished the writer with a table which he made, showing the number of men and women of the scientific fields who were mentioned in the first and second editions. Table III is published with his kind permission. Here it is seen that the field of psychology leads; the field of zoölogy comes second in respect to the number of women who are represented in American Men of

TABLE III

SCIENTISTS MENTIONED IN AMERICAN MEN OF SCIENCE

	In First Edition, 1905–6					In Second Edition, 1910					Inc. or Dec. in last 5 yrs.
	Men	Women	Total	Per cent of Whole	Per cent of Women	Men	Women	Total	Per cent of Whole	Per cent of Women	
Chem.	666	11	677	16.9	1.6	1,004	22	1,026	17.9	2.1	−0.5
Physics	664	8	672	16.8	1.8	973	12	985	17.2	1.3	+0.1
Zool.	402	39	441	11.0	8.9	715	58	773	13.4	7.5	−1.5
Bot.	385	16	401	10.0	4.0	654	31	685	11.9	4.5	+0.5
Geol.	439	5	444	11.1	1.1	509	7	516	8.9	1.3	+0.2
Math.	330	10	340	8.5	2.9	435	14	449	7.8	3.1	+0.2
Path.	351	6	357	8.9	1.7	570	9	579	10.1	1.6	−0.1
Astr.	154	6	160	4.0	3.8	164	9	173	3.0	5.2	+1.4
Psychol.	174	20	194	4.9	10.0	202	22	224	3.9	9.8	−0.2
Physiol.	98	7	105	2.6	6.6	137	5	142	2.4	3.5	−3.1
Anat.	117	1	118	2.9	0.9	95	4	99	1.7	4.0	+2.3
Anth.	85	6	91	2.3	6.6	78	5	83	1.4	6.0	−0.6
	3,865	135	4,000	100.0	3.42	5,536	198	5,734	100.0	3.45	+0.03

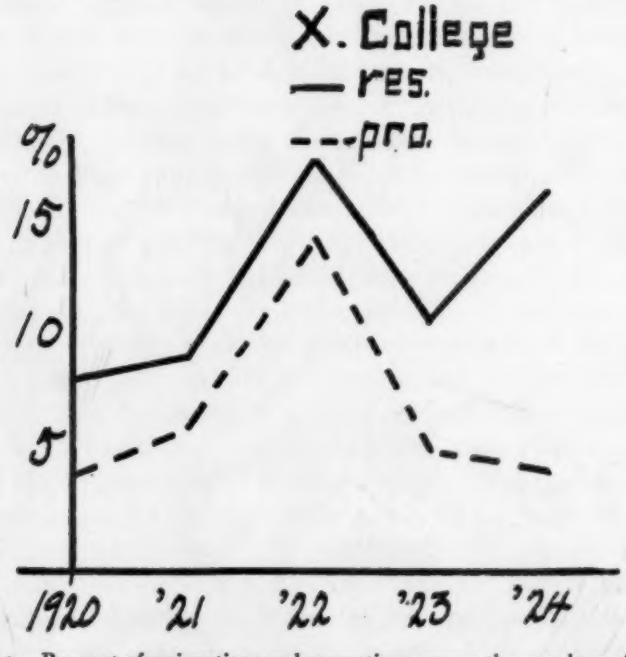

FIG. 3. Per cent of resignations and promotions among the members of the faculty of X..... college

Science. This seems to indicate that these fields not only offer good opportunity for women, but offer opportunities whereby women can rise to eminent positions.

Another item concerning which we must have information before we can have a rational vocational guidance among psychologists is earnings. Figures should be gathered which will show how much people are earning in psychological work. These figures should be compared with earnings of workers in other scientific fields. It is hoped that some tactful way of obtaining this information can be devised which will elicit the coöperation of psychologists.

This investigation, constituting a start toward the collection of facts which may be useful in instituting a vocational guidance service for psychologists, shows that there are a number of questions pertinent to the matter which can be answered if one will search for the answers with the methods of personnel research that are being developed in the 'secular' vocations. (It will be observed, by the way, that these facts were all obtained without the use of the often misleading and always obnoxious questionnaire, and it is certain there are still other facts which can be obtained in an innocuous manner.) [5]

In conclusion, we might suggest that all apart from their bearing on vocational guidance these facts are of interest to psychologists, for they furnish bases on which workers in the profession can direct their efforts more effectively.

[5] See, *e.g.*, Bingham, H. C., 'Mechanical Aids for the Classification of American Investigators with Illustrations in the Field of Psychology,' National Research Council, Washington, 1922.

CONCERNING ART STANDARDS

BY PAUL R. FARNSWORTH

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Criteria

Many theorists have attempted to settle the question of the universality of art criteria, and have in general applied similar logic to pictorial art and music. But with all the theorizing, there has been little experimental work to substantiate the viewpoints. Within the last few years, the rise of mental testing has put a practical value on this issue. If art standards are universal, then tests of what is good in art can and should be made. This is a common contention.

Much of the disagreement between the estheticians is undoubtedly due to a confusion as to art criteria. 'Agreeableness of the effects produced' is usually implied. However, 'similarity to natural objects,' 'stability of preferences' (the 'lasting' qualities of compositions), and perhaps other criteria are common. The extent to which these criteria overlap has never been determined. When the two criteria—agreeableness and stability of preference—are analyzed, it is clear that the same factors do not necessarily operate to equal extents. Monotony may predispose a 'jazz' piece to a short life, but may not so markedly effect its present standing.

Agreeableness

Meyer [1] has found that the appreciation of quarter-tone music can be developed among occidentals, but that it takes several repetitions. Since quarter-tones are not found in the current music, the rating is low until adaptation occurs. Moore [2] found the same effect with dissonances. Increasing familiarity gives higher ratings (to a certain point).

[1] Meyer, M. F., *Amer. J. Psychol.*, 1903, 14, 456–478.

[2] Moore, H. T., Psychol. Monog., 1914, 17.

Of course this does not preclude the possibility of universal standards. That there is a certain amount of similarity in bodily structures is obvious. From this, one might reasonably conclude that certain tonal or visual stimuli should call forth similar responses. Indeed, sufficient data has been gathered to convince many that there is a common substratum for music. But the discovery of a universal substratum does not imply that there is a universal criterion for pronouncing a musical composition good or bad. The organism's complicated reactions depend far more on its individual experiences. All these responses have strong visceral or emotional components which are conditioned in many ways. It is quite true that a composition may appeal to a certain mood, and be disliked a half hour later. History seems to indicate that artistic reactions will be uniform only to the extent that group experiences are uniform. Since the average individual is subjected to music far more than to pictorial art, and much more to pictorial than to plastic art, one would expect a decreasing amount of uniformity resulting from group judgments in this progression.

A study by Dr. Kate Gordon [3] bears directly on this. Dr. Gordon took fifty colored plates of oriental rugs, and had them rated as to beauty. Her raters included rug experts (both rug dealers and artists), and people of average artistic training. Her results showed that although all correlations between raters were low, those between experts were much lower than between non-experts. The criticism might be raised that if there *are* absolute standards, the rug series here presented might have fallen at the upper end—that the rugs were all excellent (although Dr. Gordon says, "The rugs appeared to have a very wide range of esthetic values"). If a series had been developed in which some samples had seemed actually hideous to the experimenter, higher correlations might have been found. This criticism may be valid.

However, the results show that there was practically no uniformity among those who were considered to be rug experts. If esthetic judgments are largely the result of

[3] Gordon, K., *J. Exper. Psychol.*, 1923, 6, 36–43.

conditioning, what else could have been expected, since training in rug judging is not a part of the education of most individuals?

A test for the appreciation of orchestral music, devised by Mohler and reported by Trabue [4] seems at first glance to point to an opposite conclusion. It is claimed for this test that it measures 'ability to distinguish between good music and poor music.' Twenty-two compositions were judged and sixteen of these were taken to form the test. These included 'Barkin' Dog Blues' and the like, which would hardly be found on the programmes of the average concert, and compositions of Schubert, Mozart, Wagner and others. Considerable uniformity was found in the ratings of musical experts (music teachers) and a fair correlation between ratings of experts and non-experts. The test was given to school children of various ages. Students of the Horace Mann School, New York City, scored highest, and showed the least improvement on subsequent training and testing.

It seems to the writer that this only tests the standards of a specific group of the present day. If similar tests had been given when Wagner first started his career of composition, his Introduction, Act III, 'Lohengrin' would have been rated without doubt even lower than it actually was rated (and it fell quite low). Since the students of the Horace Mann School, due to their environment, have far greater access to the music accepted by American music teachers than have the students of many other schools, it is to be expected that their ratings would be very much like those of their teachers. Since their ratings start high, less improvement is likely.

Stability of Preference

Gilliland and Moore [5] have worked with a different criterion in mind. Their problem was to ascertain the relative stability of jazz and classical music (the term classical is used in the popular sense). Beethoven's Fifth Symphony,

[4] Trabue, M. R., *J. Educ. Psychol.*, 1923, 14, 543–560.
[5] Gilliland, A. R., and Moore, H. T., *J. Appl. Psychol.*, 1924, 8, 309–323.

First Movement; Tschaikowsky's Sixth Symphony ('Pathetique'), First Movement; 'That's It,' a fox-trot, and 'Umbrellas to Mend' constituted the stimuli. These four compositions were played twenty five times to a group of thirty five Dartmouth students. Ratings were made on a scale of ten (ten being highest). At the beginning of the experiment, Beethoven was rated 5.88, Tschaikowsky 5.60, the fox-trot 5.00, and the one-step 4.37. At the twenty-fifth hearing, the Beethoven rating had risen to 6.09, the Tschaikowsky to 6.94, while the jazz had remained practically constant,—4.91, 4.50.

Conclusions

At just what point in esthetic theory do these researches leave us? Dr. Gordon's work with rug pictures records a failure to find standards of agreeableness which can be readily ascertained by group tests.

Dr. Trabue's claims of a scale which ascertains what is good in music have no justification at present, unless the judgment of American music teachers is arbitrarily set up as the criterion. The test will doubtless be of great worth in developing uniform reactions in school children. This is of extreme importance, as music affords opportunity for enjoyment, and an individual who does not appreciate the accepted music of the day misses much. However, many artists already bewail the tendency to stereotype music appreciation, and this test may appear to them as but another step in this movement. These two researches, then, do not disprove the contention that the agreeableness of a composition is to a considerable extent a matter of conditioning, and that universality will be approached only when environmental effects become similar.

The work of Gilliland and Moore is an attempt to demonstrate under laboratory conditions what will probably occur over long periods of time. The jazz compositions studied did not have the 'lasting' qualities of the more approved compositions. An objection might be raised against the theory

involved. The decision as to which musical compositions will remain is primarily in the hands of the gifted musicians whose musical heredity is not comparable to that of the majority of Dartmouth students.

The experiments discussed in this paper are instructive, but not conclusive in any general sense.

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